

Why Not to Test Cannikin

THE United States Administration seems bent on making everybody wait until the last minute before knowing whether there will actually be an underground explosion of a five megaton weapon in the Aleutians in October. Both the Department of Defense and the Atomic Energy Commission have said that they see no reason why the Cannikin test should not take place, but President Nixon is wisely determined to make the final decision himself. After all, it is he and not the designers of the warheads for the Sprint ABM rocket who will have to travel to Alaska two weeks from now to spend an afternoon with the Emperor of Japan. He or the Department of State, and not the technical people, will also be the first and principal recipients of any trouble which may come from the way in which the Cannikin test is likely to offend the Soviet Union and disturb the SALT negotiations in Helsinki. And finally, of course, with the Atomic Energy Commission and the Department of Defense in constant trouble on issues as various as the proper siting of nuclear power stations and the continuing war in Vietnam, it is understandable that President Nixon should find it necessary either to fortify a decision to go ahead with Cannikin by the prestige of his own office or—perhaps—to abandon the project at the highest level.

In all the circumstances, the arguments against the Cannikin test need to be appreciated for what they are—chiefly political and diplomatic but also technical. After all, the Cannikin explosion will amount to five megatons. In previous underground explosions, the AEC has moved gingerly towards one megaton. The most powerful underground explosion so far under American auspices, the Milrow test carried out in the Aleutians in 1969, was, to be sure, a fairly reasonable sign that Cannikin will do very little damage. It is improbable that the event will trigger off tsunamis. It is also improbable that the worst fears of the protesters will be realised and that earthquakes will follow the explosion. But there is a greater risk of a leakage of radioactive material from the site. No doubt the cautious fellows at the AEC have done all they can to ensure safety, but it is only as recently as December 1970 that a miscalculation led to a leakage of

radioactivity from an underground explosion in Nevada. That was only a small test, but even so it stopped the weapons testers in their tracks for close on half a year. Can they now be sure that the much larger undertaking in the Aleutians planned for October will be safe? And are they in a position to predict what diplomatic fall-out there will be if radioactive material should find its way either to the Soviet Union or to Canada?

Another and more serious issue is the extent to which such an obvious test of a warhead for an ABM may upset the negotiations at Helsinki. It is true, of course, that the issue is by no means simple. In the past few weeks it has begun to seem as if an agreement on the mutual limitation of anti-ballistic missiles would be the chief component of an agreement between the Soviet Union and the United States on the limitation of strategic weapons. The fact that a comparatively small item in the package, the agreement that there should be an improved method of instant communication between Washington and Moscow based on a communications satellite, was made public prematurely last week is a sign that there are serious difficulties in reaching a more comprehensive treaty. In circumstances like these, to be sure, it may be entirely appropriate that one potential signatory or the other should be seen to be pushing vigorously ahead with preparations for defence—if the Soviet Union earnestly desires an agreement on ABMs, testing by the United States may help to encourage an agreement. The difficulty, unfortunately, is that the Russians have been talked into believing that ABMs are bad. A show of obduracy may therefore be more a provocation than a pressure in the right direction.

It is possible, of course, that President Nixon has made an exactly similar calculation and that he intends, at the last minute, to call a halt to the Cannikin test. In that case no lasting harm will have been done. But if the explosion goes ahead as at present planned, it will be a great misfortune if the managers of the AEC are able to persuade themselves and the Administration, as they have done before (see page 230), that opposition to this event is entirely unfounded.

Creating a Professional Labour Market

THERE is in Britain a remarkable coincidence of view emerging about the steps which need to be taken to bring the output of the universities and other institutions of higher education more accurately into line with the demand. In the past few months (see page 225), pure mathematicians have been told by the Science Research Council that they must modify postgraduate courses in such a way as to produce graduates who are at once more broadly based than in the past, by which the Science Research Council implies a greater awareness of the fields of study in which pure mathematics can be profitably

applied. The fact that some mathematicians are up in arms about this advice, and are pleading that such instructions entail an infringement of academic freedom, should be no surprise. This, after all, is what physical scientists said when the Science Research Council first embarked on its policy of increasing the proportions of students on short advanced courses in the entire population of postgraduates. To some extent, the mathematicians who protest will no doubt learn a lot to their discomfort in the report, issued this week, of the Working Group on Biological Manpower (see page 224), which comes out



with an endorsement of the Science Research Council's old policy which is in turn endorsed by no less a person than Sir Frederick Dainton. On the face of things, at least, the Science Research Council will have no difficulty in winning its case. That, however, is only half the battle. It is also important to understand just why these policies are now in vogue.

The first thing to be said is that universities are by no means in scorn of academic ideals if, for some reason or another, they seek to educate students in ways which are considered a preparation for life in the outside world. It is, however, important that such a concern does not imply that higher education should consist of what is more commonly known as vocational training. Indeed, it should by now be apparent to all concerned that many of the difficulties which science graduates are experiencing in finding jobs are simple consequences of the narrow base of their education, however academic its character. Indeed, the university curriculum in science has over the years become a strange amalgam of education in the broadest sense, and training in techniques which are essential preparations for careers in professional science. The result, quite understandably, is that a great many scientists are hampered by their training from moving easily from one branch of science to another. The implications of this state of affairs for the labour market in professional science, clearly defined by the Working Group on biological manpower, are profound.

Jobs in professional science, in teaching, research or development, are bound in a relatively stable society to increase at a measured pace. The numbers of people employed in research, for example, are unlikely to grow much faster than the Gross National Product although, of course, there will be variations from one industry to another. To be sure, the pace of expansion in fields such as medicine may grow with great speed as a result of policy decisions to invest more social capital in health, but these are exceptional circumstances. And it is beyond dispute that all societies are likely in the next few years to want to increase their commitment to higher education as quickly as their commitment to any other form of expenditure. The consequence, of course, is either that courses of higher education which are vocational or partly vocational in character will grow less quickly than the universities as a whole, or that they will keep pace with the general rate of expansion, but turn out graduates unable to find jobs. The figures for British higher education are striking enough. The output of graduates of all kinds was in the late sixties growing at 23 per cent a year at a time when the conventional demand for biologists, one of the most buoyant parts of the labour market in science, was growing at only 10 per cent a year. It has been conventional wisdom for several years to say that the growth of secondary education entails changes in the school curriculum, particularly in the sixth form. So far as undergraduate education is concerned, the only novelty in the present situation is that the same effect has been translated into higher education.

By good luck, or perhaps even by good judgment, there have in the past few years been some important experiments in the design of broader university courses of a kind likely to turn out graduates who are likely to be comparatively flexible in their choice of a career, either within science as a whole or in such a way that a scientific background is considered an important part of a general

education. Without in any sense decrying the value of the boldest experiments of all, the attempts to introduce into universities thoroughly liberal courses of scientific study, much must be said in favour of more arbitrary marriages between courses of different kinds—physics and economics, for example. To be sure, in these respects British universities and other institutions of higher education are still much more narrow than their counterparts elsewhere, especially in the United States, but there is at least a chance that some of the new courses being tried out may have the advantages of breadth, and of the discipline that goes, or is supposed to go, with depth. Although in the past, universities (especially in Britain) have been inclined to think that new courses or combinations of courses can be launched after a sufficiently long period of committee meetings within the faculty, the truth is that much remains to be done before there is an objective appreciation of the advantages of different methods of educating science undergraduates more broadly. All those who pronounce on the subject, the Science Research Council and the Working Group on biological manpower included, have an obligation to say as much and, when they have the power, to spend money on this cause.

The issue of postgraduate education is more complicated. For four years, the declared policy of the Science Research Council has been to maintain in postgraduate courses a constant proportion of those taking first degrees in science and engineering, but to ensure that increasing proportions of the postgraduate students will follow courses of advanced study lasting one or two years. In other words, although the numbers of PhD graduates will continue to increase, the proportion of first degree graduates completing PhD courses will at the same time decline. It is of course important that this policy has been linked with the deliberate attempt by the research councils to divert science students towards practical studies and with the policy of selection and concentration by means of which some departments in certain universities have been encouraged to develop in directions in which they excel. Those pure mathematicians who are now protesting might equally well have cried scandal at any time in the past five years, for the intentions of the Science Research Council have been declared policy all along. Moreover, the question asked about them is not whether they entail an infringement of academic freedom whatever that may mean, but whether they are in the best interests of students and of society at large.

There is at least some reason to believe that the attitudes towards PhD courses which are now becoming conventional are too draconian. To be sure it is easy enough to argue that if undergraduate courses in British universities have a dangerously strong vocational flavour, then PhD courses, which prepare people for research and virtually nothing else, are even more restrictive. This is one of the reasons why the Science Research Council has come down strongly in favour of postgraduate courses which last for only one or two years and, with such a pattern, it is of course easy to see how the selection of PhD students might be carried out in such a way as to provide cohorts of young men and women even more dedicated to research and the ivory tower than they are at present. The flaw in the argument is of course the assumption that a long postgraduate course consisting to a large extent of original research is exclusively a prepara-

tion for a career in research or academic life. To be sure, it is remarkable that in the past few decades, even such a narrow preparation has made it possible for growing proportions of the PhD population to work at other tasks. Moreover, if a scientific education may be a good intellectual preparation for a general career, may not a properly organized PhD course be still better? This is one question that should be answered objectively before too much is done to concentrate PhD courses still more narrowly on the needs of research and the universities,

for that will only create a set of circumstances in which the assumptions underlying the new policy become self-justifying. And as at the undergraduate level, so in the graduate schools, nobody knows what kinds of curricula would exploit these opportunities most efficiently. In short, those committees which are inclined to follow the Science Research Council and the biological manpower committee in asking for smaller proportions of PhD students should first of all apply themselves to learning more about the functions of postgraduate education.

Pugwash Joins the Establishment

As the years go by—there have been fifteen of them so far—the Pugwash conferences have become respected and even respectful. The twenty-first conference, held at Sinaia in Romania at the end of August, seems to have been one of the most sober and constructive so far. Certainly the document now published by the Pugwash Continuing Committee is much more closely argued than many of its predecessors. For what the declaration of a private group of people is worth, the document also serves to bridge many gaps which have on previous occasions been contentious issues in Pugwash and elsewhere. On this occasion, for example, it seems to have been widely agreed that anti-ballistic missiles should be abandoned by the nuclear powers and that indeed the SALT agreement should not merely limit the possession of ABMs by the United States and the Soviet Union, but should actually abolish them. The conference appears to have taken a cheerful view of the decision to ban biological weapons, and the throng of signatories to the Geneva Convention in the past few months, but in pleading that governments should not therefore be diverted from the search for an agreement on chemical weapons, the delegates to the Pugwash meeting appear to have been divided—as indeed their governments are—on the need for on-site inspection. Sensibly enough, the meeting has reaffirmed its belief in a comprehensive test-ban treaty as well as its scepticism about the supposed economic benefits of nuclear explosions for peaceful purposes.

The surprising familiarity of these views does not of course imply that Pugwash has ceased to be a valuable institution. Over the years, it has demonstrated that it can at times of stress serve as a valuable channel of communication between East and West. It is a fact, even if now a comparatively old fact, that Pugwash helped enormously in the negotiation of the partial test-ban treaty in 1963 and 1964. As it has developed, the organization is no doubt regarded by all governments as a valuable listening post, to say nothing more of it. In the circumstances, however, it may not be surprising that Pugwash has fallen into one or two familiar traps, at least as far as can be told from its latest declaration. In Romania this year, there seems to have been a general feeling that political circumstances now favour an international conference on European security along the lines of the proposal being canvassed by the government of Finland. The notion is that there should be to begin with a mass renunciation of the use of force and of tendencies to interfere in other people's internal affairs, and the recognition of boundaries, renegotiated as necessary. The latest statement includes a string of detailed proposals about

the regulation of tactical nuclear weapons in Europe that might be a sensible basis on which to construct a more permanent agreement. Outside Europe, on the other hand, the statement of the Continuing Committee consists largely of hand-wringing over the war in Indo-China and the renewed threat of war in Korea. But is this not too Western a view of what needs to be done to make the world a safer place in the decades ahead? Pugwash is wise not to chase some of the hares which used to distract its attention, but is there not a strong case for keeping permanently on its agenda the search for the issues which may replace the threat of a European war in the seventies and eighties? Should not, for example, Pugwash be working hard at the design of international institutions likely to help with such things as the regulation of fish or telecommunications, two obviously contentious issues? Another trap into which there is a danger that Pugwash might fall—the latest statement from Romania skirts carefully around the edges—is the tendency to repeat clichés about the environment.

100 Years Ago



WHEN ocean cables were first submerged, various apprehensions of probable injury were entertained, some of which have proved to be well founded, and others less so. It was supposed that worms or mollusks would burrow in the substance of the envelope, and ultimately penetrate to the centre of the wires; or, again, that the attachment of barnacles, mollusks, or other marine animals on the exterior would invite the attacks of the sharks, rays, and other fish of powerful jaws, and induce them to subject the bunch of matter to such a mastication as should produce serious harm to the cable. To what extent any accidents have happened from this source it is perhaps difficult to say; but we now learn from *Harper's Weekly* that the Florida cable between Punta Rosa and Key West has been injured in numerous places, as supposed by sea-turtles biting through or crushing it in their teeth, to such an extent as to destroy its continuity. It is, perhaps, a question whether the turtle be chargeable with these operations; and we think it is quite as probable that, under the circumstances, some ray or other fish has attacked it, and for the reasons already suggested.

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OLD WORLD

ENVIRONMENT

Global Watch

from a Correspondent

Canberra, September

A FRUITFUL meeting in Canberra on September 1-3 has ensured that several ambitious and promising proposals for putting global environment monitoring on a firm scientific footing will be put to the Stockholm Conference on the Human Environment in June 1972. The proposals, prepared by working groups of the Special Committee on Problems of the Environment (SCOPE) of the International Council of Scientific Unions (ICSU), were adopted at SCOPE's first general assembly here. The chief proposal is for a world-wide monitoring system aimed at helping to detect significant and potentially harmful changes in the environment early enough to take preventive action, and eventually to predict harmful trends before they occur. To back this up, a SCOPE committee is examining methods of analysis for major pollutants in air, water and biological tissue and hopes to make recommendations on the best methods for measuring each pollutant by the end of 1972.

The proposals were prepared at the request of Mr Maurice Strong, the Secretary-General of the Stockholm Conference, who was an invited guest at the meeting. They call for the regular collection of data from a global network of a hundred or more selected areas exposed to minimal, medium and intense human impact. Every nation is being urged to set up a national monitoring office to integrate its own monitoring effort and to contribute to international coordination. Also existing monitoring activities would be fed into the system.

The working group listed three sets of problems which deserve the highest priority for global monitoring: potentially adverse climatic changes resulting from human activities; potentially adverse changes in biota and man from contamination by toxic substances; and potentially adverse changes in biological productivity caused by improper land use. Highest priority is given to atmospheric measurements of turbidity, carbon dioxide, surface solar radiation and standard meteorological variables; monitoring mercury, lead cadmium, DDT and polychlorinated biphenyls in air, water, soils and biota; keeping check on a variety of data reflecting the state of human health; and implementing studies of vanished or endangered ecosystems and vertebrates and of the population size and distribution of birds. The documents say that research activities and pilot studies are essential for defining and evolving detailed monitoring programmes where necessary, and

recommend the establishment of internationally funded and staffed laboratories in at least three monitoring areas subject to minimal human impact.

Mr Strong told the assembly there had been a gratifying response to requests for contributions to the conference from governments, United Nations agencies and scientific organizations. He predicted that the main constraints on necessary environmental projects after Stockholm would be political unwillingness on national levels, not a lack of sufficient ingredients for action. Mr Strong also said that another conference, similar to that at Stockholm, might be needed within three or four years, and argued a case for continuing and consolidating the present organization for Stockholm—in effect advocating a new UN agency for the environment. This proposal runs counter to the views of the existing UN agencies with fingers in the environmental pie (for example, UNESCO, FAO, WHO) and the scientific unions, who see it as, on the one hand, a threat to their existing responsibilities and, on the other, as an unnecessary proliferation of international bureaucracies. In pressing his case, Mr Strong said "I don't think we are at a sufficient state of readiness in terms of knowledge and particularly political willingness, to do all the kinds of things that are going to be necessary".

BIOLOGICAL MANPOWER

Broadening the Base

PROFESSIONAL biologists are the latest to be told that their education should be broader and that they must be more ready to change disciplines in mid-career. The *Report of the Working Group on Biological Manpower* (HMSO Cmd Paper 4737, £0.85), published this week, also echoes previous declarations by, for example, the Swann committee (of which it is a descendant) that qualified scientists must broaden their concept of what constitutes a professional job as the output of the universities and other institutions of higher education grows so as to outstrip what is at present considered to be the demand. The working group, the chairman of which is Dr Richard D. Keynes, director of the ARC Institute of Animal Physiology at Cambridge, is, like other bodies, anxious that university departments and grant-giving bodies should in future discriminate more carefully between graduate students who can make proper use of a PhD course and those who have less aptitude for original research.

Sir Frederick Dainton, chairman of the Council for Scientific Policy, is one who plainly finds the group's recommendations to be acceptable. In his introduction to the report, he says that

"much of the report could be applied to science in general". Taking his cue from his own report on the subject, now five years old, he goes on to say that "it will have to be recognized increasingly that on the one hand a specialist scientific education will not guarantee employment at a professional level in the subject of a graduate's specialization" and that graduates in science have valuable jobs to do outside the confines of their profession.

The members of the group are as follows: Dr H. O. J. Collier, Mr D. J. B. Copp, Dr F. A. E. Galley, Professor J. M. Mitchison, Professor S. J. Pirt, Professor M. M. Swann and Professor C. P. Whittingham. Apart from statistics which exist already, the working group had access to the results of employment surveys carried out within the research councils and a questionnaire survey within industrial organizations.

By 1968, the 10,918 biologists employed in Britain accounted for more than ten per cent of all scientists, but their numbers were growing more quickly, at an annual rate of 5.5 per cent, compared with 4.4 per cent for scientists as a whole. Understandably, most biologists appear to be teachers at some level, 8,100 of them in schools, universities and further education. These calculations, of course, omit those who work in medicine, dentistry and health, more than 95,000 in 1966.

The essence of the employment problem identified by the working group is that the annual growth of the production of first degrees in biology is accelerating and amounted to 19.9 per cent per year in the late sixties, while the annual percentage growth of biologists with higher degrees amounted to 21.7 per cent per year. These rates of growth are greater than the corresponding rates in other branches of science, but not markedly so. On the other hand, the number of extra jobs likely to arise each year by natural causes is estimated to be something between rather more than 1,000 and rather less than 1,200 qualified people, or roughly ten per cent of the working population of qualified biologists. Either the remaining ten per cent have to be unemployed or have to find jobs elsewhere.

On the broadening of the curriculum, the working group says that this is the best way of minimizing "the undesirable effects of an imbalance between the supply of biological graduates and the demand for their services in specifically biological fields". It recognizes that one consequence must be more mathematics and physics in the training of biologists at all levels. The working group is a little heartened to see that only 33 out of 44 universities now offer separate courses in botany and zoology. At the postgraduate level, the working

group is a little taken aback to find that employers seem to prefer what it calls "conventional" postgraduate courses but unabashed, and suspecting that the predilections of government research establishments have had an undue influence in previous inquiries, the working group goes on to say that the PhD course should be improved. It fears that too many specialist PhDs might mean "individual frustration and unhappiness" and asks for bold experiments, for more stringent selection for PhD courses and for graduate courses aimed both at providing professional people with up to date information and with matching the education of biologists to the non-professional jobs which they will increasingly be required to take.

MATHEMATICS

Pressure of the Purse

MATHEMATICIANS in Britain are concerned that academic freedom may be infringed by an apparent change of policy by the Science Research Council Mathematics Committee. A protest on the subject from Professor W. B. Bonnor of Queen Elizabeth College, University of London, appeared in *The Times* last week. He complained that the SRC is instituting standards other than those of academic excellence in allocating research studentships.

The issue has been brewing since March this year, when heads of mathematics departments at British universities were told in a letter from the secretary of the SRC Mathematics Committee of anxiety about career prospects for MSc and PhD graduates in pure mathematics, many of them pensioners of the SRC. The committee said that in the next few years, research in pure mathematics is likely to offer fewer opportunities for jobs than the number of students being trained and that in the circumstances it would be prudent for mathematics departments to broaden the training given to graduate students by means of courses in applied mathematics, theoretical physics, engineering subjects, computer science or business economics. In March, mathematics departments were asked to let the Science Research Council's committee know how they were responding.

Fuel was added to the flames of protest at the end of June by a second letter asking that within twelve months the committee should be told what had been done and saying that it would then "hope to feel well-enough informed to be able to reflect departments' efforts in next year's quotas of studentships".

Professor Bonnor sees this as an attempt to dictate how mathematics should be taught at postgraduate level. He said last week that "although a case

can be made for what the SRC is trying to do, there is no justification for making mathematics departments carry it out by threatening their financial support." He also said that the SRC's suggestions were ill-conceived. First, a course of twenty or so lectures on any of the suggested topics would not help students to find jobs. Second, such courses of lectures would lower the standard of the PhD degree. Third, courses would not make students more valuable to employers. Professor Bonnor is only a little more cheerful about intensive courses after completion of the PhD course. He emphasizes that the only criterion that should determine the support for a university department is the excellence of its research.

A written explanation from Sir Brian Flowers, chairman of the Science Research Council, to Professor Bonnor says that "the SRC would not dream of dictating to mathematics departments how they should do their research or teach their postgraduate students and I cannot see that anything that has been written implies this; however, we do hope to encourage mathematics departments to think about the design of their postgraduate activities in the light of the fact that many of the students almost certainly will eventually be seeking employment outside the purely academic field".

Heads of other mathematics departments share Professor Bonnor's views but less strongly. Professor G. Higman, University of Oxford, said last week that his department had answered the letters but that unemployment of Oxford PhD mathematics students is not a problem—general courses for mathematicians are, however, already offered at Oxford. Professor D. J. Bartholomew, University of Kent, is concerned about the implications of the letters and feels that as a matter of principle the SRC should not dictate to departments on matters of curriculum. Professor H. B. Griffiths, University of Southampton, said that the SRC request does not necessarily infringe academic freedom but that the mathematical postgraduate teaching at Southampton had already been "broadened".

The Association of University Teachers is particularly interested in Professor Bonnor's claim and it is on the agenda for discussion at the AUT's meeting in Bath today. Dr Sheila Brenner, senior vice-president of the AUT and chairman of the AUT's education committee, said earlier this week that "although the AUT expects graduates at both first and higher degree level to tackle a variety of problems in industry and other occupations, it is not the university's function to give them job training." She went on to say "Our general opinion is that external direction to educate students in particular ways may well prove counter productive in the

short run and in the long term will be damaging to the national interest".

Dr Brenner also said that "many university departments are under considerable financial pressure at the moment and while we would welcome additional funds for those departments which have plans for experiments to improve the education they offer their students, we shall watch carefully to see that this is not done at the expense of departments which do not consider a particular pattern of education appropriate."

RESEARCH AND DEVELOPMENT

No Wolf at the Door?

DR BASIL BARD, managing director of the National Research Development Corporation since July, seems unperturbed at the Docksey Inquiry for the Department of Trade and Industry into the NRDC's arrangements for the exploitation of inventions. The inquiry could mean that the NRDC's days are numbered, but Dr Bard, speaking after the publication of the corporation's Twenty-second Annual Report (HMSO, £0.45), said that until the results of the inquiry became known towards the end of the year, the NRDC was carrying on as usual and that he had "no reason to believe that we won't carry on substantially as usual" after the results become known, with possibly some changes of direction and emphasis. In the report Dr Bard welcomes the inquiry, and says "it seems to us timely that the corporation and its work be reviewed in the light of the present-day and future requirements of the country", adding that "the salient figures for the past four years offer in our view a reasonably encouraging picture" of the corporation's work.

The report shows that the corporation's income exceeded £6 million for the first time and that its revenue account showed a small surplus (£68,960) for the third successive year.

The activities of the corporation expanded during the year with 96 new development projects being undertaken and with 404 projects receiving support compared with 377 in the previous year. Of these projects 186 are joint ventures with industry. The corporation's investment in development projects rose from £5.8 million in 1969–70 to £5.9 million last year, and recoveries from the projects amounted to £2.4 million last year compared with £1.3 million the year before.

The corporation has a borrowing limit from the Department for Trade and Industry of £50 million with which to finance projects, and £24.8 million of this was in use to September 1971, £1.3 million more than at the same time last year. The actual annual borrowing for the year fell from £4.88 million in

1969-70 to £2.85 million. Licence income from patents, the corporation's biggest source of income amounted to £3.7 million, of which £2.7 million were foreign currency earnings. The drug cephalosporin C was again the largest revenue earner.

Submissions of new projects for support rose from 2,194 to 2,535, but the numbers presented by the universities and industry both fell, the increase coming from submissions by private inventors. The corporation felt that the fall in applications from industry was "a possible reflexion of the cutting back of research and development by firms encountering cash flow difficulties". Commenting on this Dr Bard said that he was convinced this was "only a hiccup" in the growth of research and that it would be "the greatest mistake at the present time to cut down on research and development".

RESEARCH AND DEVELOPMENT

Small Firms Feel Pinch

A REPORT ON *Scientific and Engineering Manpower and Research in Small Firms* (HMSO, 40p), published last week, concludes that the use of scientific and technical personnel in small companies may be linked to the capital intensiveness of the industry concerned.

The report, one of the research studies commissioned by the Bolton Committee of Inquiry on Small Firms, sets out to examine the degree to which small companies—those with between eleven and 200 employees—are in a position to benefit from, and contribute to, the technological advance of British industry.

The report shows that small companies employ about a third (2.5 million) of all employees in manufacturing industries, but only 15.5 per cent of the equivalent of graduate qualified engineers, technologists and scientists (QSEs). (Figures are for 1968.) This is a proportion of QSEs to other employees of 0.7 per cent, only half that of larger companies. The distribution within small companies is not even, but small electronics companies alone have a higher proportion of QSEs than their larger competitors. The proportion of QSEs in small electronics companies is higher than in other small companies because research in electronics is a rapidly advancing technology at a highly theoretical as well as a purely practical level, and because "materials are relatively inexpensive and large capital facilities are unnecessary". Innovation in fields such as mechanical engineering requires a bigger "technological step" and large expenditure on the trial and testing of new products.

This led to the conclusion that capital-intensiveness may be the key to

QSE employment in small companies, and the observation that many small companies rely on contracts with outside laboratories for their research work as this limits the extent of their expenditure and removes the necessity of maintaining their own full-time research facilities.

The expected growth rate for small companies from 1968-71 was 27.1 per cent compared with 20.9 per cent for all companies. If the report's suspicions about a "technological step" are right, then this "implies a barrier to greater employment of QSEs and further technological advance".

MUSEUMS

Timbers to Greenwich

from a Correspondent

THE National Maritime Museum (NMM) at Greenwich is advertising for an archaeologist to be appointed at the relatively elevated rank of Assistant Keeper. This is certainly novel, and may be thought bizarre, but it is the direct result of the archaeological "rescue" of the Graveney boat unearthed in Kent salt marshes last October (*Nature*, 228, 402; 1970) and since then carbon-dated by the British Museum as late ninth century.

The new director of the museum, Mr Basil Greenhill, has a number of sweeping plans for a new look for the NMM and one of them is to extend the exhibition of boats on display. Apart from the *Cutty Sark* nearby, the museum has relied on its superb collection of contemporary models and numerous fine marine paintings to demonstrate the art of shipbuilding. At present its largest gallery, the Victorian Neptune Hall, is being converted to accommodate boats of different periods, not all of them small, such as the 1900 paddle-wheel tug *Reliant* whose fore-end including the bridge and engine room will form the centrepiece of the display when the hall is reopened next year. The opportunity of adding a pre-Conquest boat to the group was too good to miss although, by any estimate, the Graveney boat is no beauty.

Very considerable expertise is required to derive the full information retained in the lines and timbers of an antique vessel especially when it is sodden with salt water. The expectation is that the successful candidate, whether his or her training is in archaeology, anthropology or naval architecture, will have to acquire a number of other special skills. There is nowhere to train in the field of nautical archaeology, so the unit's archaeologist will have to resort to self-improvement. Besides looking to the eventual display of the original Graveney boat in Neptune Hall, there is a plan to build a replica and

row it across the North Sea as the Anglo-Saxons did when they journeyed between Kent and the Continent. Altogether some £50,000 has been committed to this unit.

Once the unit is in existence it will obviously do more than simply process the Graveney boat; a committee of experts will screen other candidate timbers or craft for treatment and preservation. This will provide a unique facility for Britain. There are few other places in the world where waterlogged wood up to 50 foot long can be handled. Will there be a rush of old timbers to Greenwich? One thing is clear—the line is being drawn at boats, and specifically boats less than 50 foot long. There is no possibility that the eighteenth century East Indiaman, *Amsterdam*, buried at Hastings, or Henry V's *Grace Dieu*, still in the mud of the Hamble river, will be imported to the Greenwich tanks.

EDUCATION

Computing in Schools

THE view that teacher training is the most urgent need in the expansion of computer science teaching in secondary schools is put forward in a report of an international seminar held in Sevre during March, 1970 (*Computer Sciences in Secondary Education*, OECD, Paris; 1971; £1.90).

The report, like the papers presented at the six-day meeting, recognizes the need for several types of school computing course, which could be taught by teachers with quite different educational backgrounds. These include appreciation courses which require almost no computer time and could, in principle, be taught by any numerate teacher, and other courses which contain more instruction in programming and in the working of computers; these would have to be taught by mathematicians or scientists. Ultimately, says the report, the aim should be to include at least a computer appreciation course in all secondary school teacher training.

Much of the seminar was taken up with discussions of the significance and objectives of the teaching of computer science in schools. There was agreement that one of the important functions of computer education is the encouragement of skills in organizing and handling information, which are useful in the study of most subjects and which help to convince pupils that information is a means to an end but not an end in itself.

Even the simplest appreciation course requires a certain amount of access to a computer, if the subject is not to appear to be too abstract, and the report suggests that agreements should be encouraged which allow pupils' pro-

grams to be run on computers belonging to local authorities, universities and companies. Twenty of the 160 local authorities in England and Wales, for example, encourage the use of their management service computers for educational purposes and about 30 per cent of schools which run computing courses make use of these facilities.

AIRPORTS

Up, Up and Away

For the first time, each of the four airports of the British Airports Authority has shown a net profit over a financial year—that ending March 31, 1971. Taken together, Heathrow, Gatwick, Stansted and Prestwick have been profitable since 1962, but only because the large profits at Heathrow have more than compensated for losses elsewhere. Although the total profit in the past financial year is seven per cent less than the record of £9.5 million in 1969–70, there is obviously merit in the even spread of profit. Edinburgh airport, handed over to the authority on April 1, 1971, is also budgeted to show a small profit in the current year.

Apart from economic considerations, recent months have been marked by the controversy over the siting of the third airport for London, and the final decision, made on environmental grounds, in favour of Foulness. This decision is discussed in the recently published annual report and accounts of the British Airports Authority (HMSO). The authority's view is that none of the four sites on the Roskill Commission's short list is really suitable for a major new airport and that of the four, Foulness is, on economic grounds alone, the least suitable.

Other environmental issues also appear less disheartening than when the 1969–70 report of the authority was published. The noise problem remains, but it has been shown that jet engines are much cleaner than those of surface traffic, so that in the past year—the first in which jets accounted for more than half of the world's airline fleet—airports had less concentrations of pollution than ordinary urban districts. Concentrations of smoke and sulphur dioxide recorded at Heathrow in the summer of 1970 demonstrate this dramatically (see Table).

What of the further growth of air traffic, and the difficulties of coping with this without damaging the environment? Between 1960 and 1970, the number of flights to and from British airports grew from 402,000 to 607,000, while the average number of passengers per air transport movement increased from 25 to 52 as the size of average loads increased. The authority expects

Table 1 Comparative Average Concentrations of Smoke and Sulphur Dioxide in the Atmosphere at Heathrow, Summer, 1970

In Westerly Winds	From the West (outside the airport)	From the Airport
Smoke	16	15
SO ₂	152	59
In Easterly Winds	From the East (outside the airport)	From the Airport
Smoke	29	26
SO ₂	208	91

that the average load per flight will continue to increase by six per cent a year throughout the 1970s, and that by 1985 the airports of the UK will be handling, on a "middle" forecast, 130 million passengers, 5 million tons of cargo and 1.1 million flights a year. In 1970, the

equivalent figures were 32 million passengers, 0.5 million tons of cargo and 600,000 flights. It will, indeed, be a "substantial task" to meet these demands, even if the Foulness development lives up to its proponents' greatest expectations.

Miscellaneous Intelligence

BRITAIN'S problems of what to do with some of its graduates and school-leavers do not seem to be shared by the Soviet Union. Mr Boris Kostin, head of the Moscow Employment Board claimed in a recent interview in the Soviet press that no school-leaver in the Soviet Union has any difficulty in getting a job. Those without any professional skills are trained on leaving school for several months at half pay, and a wide choice of jobs awaits them upon completion of training. Moscow in particular has an "insatiable" demand for workers in the service spheres, and for clerical and municipal workers. Moscow's largest construction company alone is building more than 100,000 flats a year.

BETWEEN 1960 and 1970 the number of visitors to those of Britain's principal monuments in the care of the Department of the Environment almost doubled from 6,800,000 to 12,500,000 and total fees received quadrupled to almost £1.1 million, a sizable part of the department's 1970 budget for ancient monuments of £2 million.

These figures were quoted by Dr A. J. Taylor, Chief Inspector of Ancient Monuments and Historic Buildings, in Hamburg, where one of the department's two touring exhibitions of "Historic Monuments in Britain" is showing this week. The two exhibitions have toured Britain and the mainland of Europe (the second exhibition is now at the Banqueting House, Whitehall) and the exhibition now at Hamburg will go next to Tokyo.

SCIENTISTS from the Soviet Arctic and Antarctic Institute have been studying the movement of glaciers in the Antarctic by means of laser beams. The experiments were carried out in the Molodezhnaya station area on the coast of the Sea of Cosmonauts and

were implemented by installing a 20 cm diameter mirror on a glacier. The glacier was then tracked by a reflected laser beam and it was concluded that its movement was random—movement occurred sometimes within seconds and sometime within hours.

THE fossils of fifteen dinosaur eggs found at Ologoi Ulan Tsab by the 1968–69 Soviet-Mongolian Geological Expedition bring the number of types of dinosaur eggs found in the Gobi desert to eight. All are ornithoid in structure, the Ologoi Ulan Tsab eggs having a diameter of 14–15 cm with an estimated fluid content of 1.5 l. The eighteen amino-acids identified by chromatographic analysis suggest that the shell membrane was basically keratin, as in modern reptiles. A comparison of these eggs with the testudoid dinosaur eggs found at Aix in France should provide data on the evolution of the different orders of dinosaurs and their relationship to other fossilised and modern vertebrates.

RESEARCH in the USSR has shown that the migratory isabelline wheater (*Oenanthe isabellina*) is a carrier of bubonic plague. The bird nests in rat holes and it has been found that its fleas feed as readily on the blood of rats, notorious plague carriers, as on their original hosts which they inflict upon returning to them. It was found during studies that birds so infected lived on for more than ten days, which presents a serious problem in controlling the spread of plague as the migratory birds would travel some distance in that time. The discovery helps account for the resurgence of the plague in areas of Russia where anti-rodent programmes in the early fifties had successfully banished the disease for some years.

NEW WORLD

Yet Another Prescription for Cancer

by our Washington Correspondent

THE National Institutes of Health may yet be rescued from dismemberment by the setting up of an independent cancer research authority. The proposals, which have so far achieved the dubious distinction of almost uniting the Senate in support and the biomedical community in opposition, last week entered a critical stage in their passage through Congress when they became the subject of hearings before the House Subcommittee on Public Health and Environment. But just as it seemed that members of the House of Representatives, like their colleagues in the Senate, would be placed in the unfortunate position of either voting for a measure likely to break up the National Institutes of Health or of recording their dissent and being accused of refusing to support cancer research, Mr Paul G. Rogers, chairman of the Health Subcommittee, offered a solution that may preserve both the structure of NIH and the integrity of Congressmen.

With the support of five members of his subcommittee, Mr Rogers last week introduced a bill designed to give the National Cancer Institute authority for coordinating all the activities of the NIH related to cancer research, and a budget of \$400 million for 1972, rising to \$600 million in 1974. The bill, which was written with the help of cancer researchers, also keeps the National Cancer Institute firmly within the NIH.

Before Rogers had introduced his bill, it seemed likely that the House would follow in the footsteps of the Senate by voting through a bill which in effect would take the National Cancer Institute outside the NIH umbrella and place it in an agency operating under the unfortunate and misleading name of the Cancer Cure Program. The Senate gave its blessing to the creation of the agency by a vote of seventy-nine to one, and since the bill also bore the sponsorship of the Administration, it looked set fair to sail through the House and into law with the opposing voices of the biomedical community drowned in the political rhetoric.

As passed by the Senate, the bill would set up the Cancer Cure Agency as an "independent agency within the National Institutes of Health" and would endow it with "such sums as may be necessary". The agency would have its own budget, a director at the same executive level as the director of

the National Institutes of Health and its own bureaucracy. To call it an integral part of the NIH on the basis of its geographical location in Bethesda would therefore clearly be a misnomer, and any doubts about the subsequent bad health of the national institutes if the bill were to become law should have been swiftly cast aside when the National Heart and Lung Institute asked for similar treatment. There is little doubt in Bethesda that the Senate version of the bill would signal the break-up of NIH if it were passed unchanged by the House.

In contrast, the bill introduced last week by Congressman Rogers and his colleagues is designed to represent as closely as possible the views of the biomedical community, but at the same time it makes a few obvious concessions to the thinking that lay behind the Senate's adoption of the Cancer Cure Program. If adopted, the bill would give the National Cancer Institute more independence and considerably more dollars than at present, and make it better known, but its standing in the National Institutes of Health's bureaucracy would not be greatly different from that of other institutes.

The key strategem in Rogers's defence of the NIH is to seek to buy off other proposals with the promise of \$400 million to the National Cancer Institute for the present year—more than \$60 million above the amount appropriated by Congress last month, which itself included an extra \$100 million requested by President Nixon in May. The Rogers bill would therefore distribute Congressional largesse to the National Cancer Institute at almost double the rate for 1971, and by 1974, the NCI would have nearly three times as much money as it is now accustomed to spend each year. Even this bid may, however, be too low to succeed in tempting the supporters of a separate agency to abandon their ideas, for the original proponents of a cancer cure agency were asking for funding to reach \$1,000 million by 1976.

As for the bureaucratic arrangements for cancer research within the National Institutes of Health, the Rogers bill would promote the director of the NCI to the level of Associate Director of NIH. And before other agencies have a chance to complain that they would be relegated to the status of poor relations, the bill similarly promotes the directors of the National Heart and Lung Institute and of the National

Institute of Neurological Diseases and Stroke and makes all three positions political appointments directly from the White House. The grouping together of these three institutes is suggested on the basis that, between them, cardiovascular diseases, stroke and cancer account for about 75 per cent of deaths in the United States, and it firmly underlines Rogers's belief that the political dust kicked up during the debate about cancer research should not be allowed to obscure the legitimate claims of other areas of biomedical research. Ironically, however, previous suggestions that the director of the NCI should be a political appointment have come under fire for placing unnecessary political ties on biomedical research, and the move to extend this provision to other institutes will do little to head off those criticisms.

Perhaps the most important provision in the Rogers bill is that the budget of the National Cancer Institute should be submitted directly to the President for review and referral to Congress. Although the Director of NIH, the Secretary for Health, Education and Welfare, and the National Cancer Advisory Council would have a chance to look at and make comments on the NCI budget, they would be constitutionally powerless to alter it. This proposal in effect may help to turn the cancer budget into a political football, to be debated separately in Congress each year, but it will also keep public attention focused on the efforts of Congressmen to help find a cure for cancer—a sure way of ensuring that funding for cancer research is maintained at a high level.

This proposed budget mechanism, although generally regarded as a lesser evil in comparison with setting up an entirely separate agency, will present a few difficulties for the National Institutes of Health. For one thing, the flexibility built into the present system, whereby budgets for the separate institutes are channelled through the director of the NIH, would be lost if the Rogers bill were put into effect. At present, revisions are made in each institute's budget right up to the time that the Administration's budget goes to press. But if the NCI's budget is to be reviewed by the Director of NIH, the Secretary of HEW and the National Cancer Advisory Council, not only must it be drawn up a good deal earlier but it would also not be open to last minute change. One official of the

NIH described this arrangement last week as a "formula for chaos", but Rogers's supporters defend it on the basis that it will assist the NCI in its planning if it can be sure that its budget request is transmitted to the President without arbitrary alterations designed to make the NIH's budget request fit the global sum allowed by the Office of Management and Budget.

Another provision in the Rogers proposals that has been billed as a means of speeding up grant applications, and therefore, by implication, the development of a cure for cancer, is the suggestion that all the directors of the research institutes should be able to distribute grants of less than \$20,000 without first seeking approval from the National Cancer Advisory Council. In the past, the advisory council has met only three times a year, and this has led to accusations of unnecessary delays in the processing of grant applications. But the force of the new proposal is probably more rhetorical than actual, for it will only speed up grant applications by at the most one or two months in a process that now takes between six and eight months, and there are no suggestions that researchers and equipment are standing idle during the application process.

The proposed arrangements are, nevertheless, being welcomed for two reasons. First, they will take some of the burden of reviewing straightforward and minor grant applications from the shoulders of the National Cancer Advisory Council, and make it less of a rubber stamping body. For another, if the proposal is accepted, it would do little damage to the established review processes of the NIH. NIH officials are naturally suspicious of any change in the grant review process, and they are particularly concerned to preserve the scientific peer review, but they accept that the force for change has probably gained irresistible momentum. The Rogers bill is regarded as doing the least damage since it applies to about 26 per cent of the NCI's grants, and in any case to the grants which usually generate the least controversy.

What chance has the new bill of succeeding where previous attempts to inject some rationality into the debate about cancer research have failed? One of the chief obstacles in the path of the bill is clearly the fact that an independent NASA-style agency dedicated to finding a cure for cancer makes an ideal subject for political hyperbole—the Senate debate on the proposal provides a good example—while a proposal in effect to give more money to existing institutions lacks the same lustre. Another difficulty facing Rogers and his supporters is the fact that the Senate has already passed the Administration's bill designed to give

the National Cancer Institute independent status, and whatever the House passes must be amalgamated with the Senate bill in a conference committee. Although members of the biomedical community can find many acceptable elements in the new bill, it is already undoubtedly a compromise with the Senate position, and may become badly emasculated in the conference committee.

Whatever compromise is finally worked out by the committee and by the House, the new bill has placed the Administration in an embarrassing position, for when the Administration's bill was first introduced, it advocated elevating the National Cancer Institute to a bureau, but keeping it firmly within the bureaucracy of the NIH. The Administration's original proposal was designed to head off attempts to create a separate cancer agency that were embodied in a bill introduced into the Senate by Edward M. Kennedy. The Administration found it politically expedient, however, to compromise with Kennedy's supporters by agreeing to a compromise bill that retained the essence of Kennedy's proposals, but which bore the Administration's name. The introduction of Rogers's bill has therefore put the Administration in the situation of defending a position which it had previously attacked, against proposals which it once supported.

DETERGENTS

Algae Better than Burns

by our Washington Correspondent

THOSE who take a delight in watching bureaucratic somersaults will have been particularly amused last week to witness three government agencies vigorously backtracking on their policies for controlling phosphates in detergents. Not so amused, however, are detergent manufacturers, who have spent millions of dollars seeking alternatives to phosphates only to be told that the cure is worse than the disease; housewives, who have switched in droves to the low phosphate detergents in their desire to do something for the environment; and congressmen who have submitted bills designed to force detergent manufacturers to remove phosphates entirely from their products. Placed in an even more invidious position are those counties and states that have banned the use of detergents containing phosphates, only to be told last week to reconsider their policies.

What caused the Environmental Protection Agency, the President's Council on Environmental Quality, the Food and Drug Administration and the Surgeon-General to declare last week that although phosphate detergents are the largest single cause of eutrophica-

tion of waterways, they should be used in preference to other detergent types? The answer is simply that substitutes for phosphates are undesirably toxic. The problem is that legitimate concern over deterioration of the environment has reached such a pitch that environmental groups are blessed with the political force to influence legislators and with enough consumer backing to pack a painful punch against industries that can be shown to abuse the environment, and this irresistible force is apt to produce overhasty reactions. As far as detergents are concerned, the environmentalists' punch forced manu-

Guide to the Bill

THE bill introduced into the House last week by Congressman Paul G. Rogers and five members of his Subcommittee on Public Health included the following major provisions:

- The National Cancer Institute would be preserved as an integral part of the National Institutes of Health, and it would be charged with the task of coordinating all the cancer research in NIH.

- The National Cancer Institute's budget would be transmitted separately to the President, but the Director of NIH, the Secretary of Health, Education and Welfare and the National Cancer Advisory Council would have the chance to comment on, but not alter it. The NCI would be authorized to spend \$400 million in 1972, rising to \$600 million in 1974.

- Directors of the Heart and Lung Institute, the Institute of Neurological Diseases and Stroke and the National Cancer Institute would all be promoted to the level of Associate Director of the NIH, and all would be direct appointments by the White House.

- All the institutes would be able to approve grants of less than \$20,000 without having to seek approval from the advisory councils, although scientific peer review for all grants must be maintained. It is hoped that this would help prevent the National Cancer Advisory Council from being bogged down with minor decisions.

- Finally, so that Congress can keep a check on the way the provisions are working, the director of the National Cancer Institute is required to present an annual report to the President for transmittal to Congress.

facturers into producing a product that provides less of a hazard to the environment, but more of a hazard to people.

The scenario leading up to the abrupt policy reversal of federal agencies last week is an object lesson in how the lobby system can overreact to produce a result that is acceptable neither to the harassed manufacturer nor to the concerned consumer. As evidence began to accumulate about the contribution of phosphate detergents to the eutrophication of lakes and rivers (the stimulation of plant life at the expense of the oxygen supply to other aquatic organisms), they became an ideal object on which environmentalists could vent their anger. A congressional committee in April last year recommended that detergent manufacturers should be made to remove phosphates from their products by 1972, the Canadian government banned phosphate detergents from that date, several states and counties, including Indiana and New York, outlawed the sale of phosphate detergents, and housewives began to switch to detergents labelled as phosphate-free. In addition, countless other state legislatures have bills pending before them, enjoining manufacturers either to remove or severely restrict the amount of phosphate in their detergents.

Flailed by the public and Congress, detergent manufacturers set to work developing alternatives to phosphates in detergents—the chief role of phosphates is to soften the water. The most promising substitute seemed to be nitrilotriacetic acid (NTA), and chemical manufacturers were hard pushed to meet orders from the detergent manufacturers. But doubts about the carcinogenicity and teratogenicity of NTA, first voiced by Dr Samuel Epstein, then a pathologist at Harvard, led eventually to a voluntary ban on the substance by detergent manufacturers in December last year. That ban was upheld last week by the federal agencies charged with protecting public health, chiefly on the basis of a study carried out at the National Cancer Institute, which produced no direct evidence of carcinogenicity at low dose levels, but which nevertheless failed to remove the doubts.

After that setback, manufacturers began to substitute caustic materials—such as sodium hydroxide—in place of phosphates. This led to action by the FDA, forcing some products to be labelled as toxic. Detergent manufacturers protest that they cannot produce a non-toxic substitute for phosphates, and legislators were continuing to force them to produce phosphate-free detergents. Finally, the Environmental Protection Agency, the Council on Environmental Quality, and the Surgeon-General decided last week to advise the housewife to choose detergents containing phosphates.

AEC

How To Sell Cannikin

by our Washington Correspondent

It has been estimated that Washington boasts more public relations personnel per square mile than any other city in the world. While most of these are hired to impress their clients' views on the Federal Government, the government itself has a small army of information officers whose role is to keep the public informed of departmental and agency projects and policies. The difference between the two groups is that the lobbyists are expected to deal in propaganda, while the government public relations officers are expected to deal in facts.

That at least is the theory, but few people accept it as the fact. A particularly fine example of how one particular government agency, the Atomic Energy Commission, set about selling a potentially distasteful project, the explosion of a nuclear weapon under Amchitka Island in 1969, and how it intends to use the public relations experience gained in that exercise to sell the Cannikin project, was uncovered last week by Senator Mike Gravel of Alaska. Gravel, a vociferous opponent of the Cannikin nuclear test scheduled to take place under Amchitka Island in October, published in the *Congressional Record* an internal AEC report describing how the AEC "is running a well-oiled, well-financed, well-coordinated machine to tell Alaskans and other Americans about the need for Cannikin".

Gravel's chief complaint is that the AEC report provides ample evidence that the commission dismissed out of hand the views of its opponents by suggesting that their opposition to the test was motivated by a desire for headlines, disillusionment with the political system or by opposition to the ABM. The AEC, he complains, "discounts all legitimate criticism of its programs" and uses tax dollars to support an illegitimate propaganda campaign. "I call this situation to the attention of the Senate, not to beat the ghosts of past errors out because the issue is very much alive today", Gravel declared. "Cannikin is upon us and the selling job is all too familiar."

The report itself describes how the AEC's public relations effort for the Milrow test in 1969—the nuclear explosion that was designed as a proving shot for the five megaton Cannikin test—was planned in three stages. The first, preliminary stage started in December 1966, and gradually picked up momentum during the two and a half years to July 1969. The chief object of this phase of operations was to inform the public that the AEC was

conducting a feasibility study of Amchitka to explore the possibility of using the island for nuclear testing. "Throughout this period", the report points out, "policy did not permit confirming that a nuclear test would be detonated on Amchitka. Public statements were framed in such language as 'determining the feasibility of Amchitka for possible use in underground nuclear testing'". The policy, however, became less than credible when it had become widely known that the AEC had let a \$12 million drilling contract, but the public relations team nevertheless stuck to its original story.

From July 18, 1969, when the AEC finally let it be known that an explosion was planned to take place under Amchitka, to October 2, when the Milrow test took place, the report describes an intensive build up in its public relations effort. Until September 20, the chief effort of the AEC's propaganda was directed towards building up public support and good will. An office was set up in a hotel in Anchorage, films documenting the AEC's efforts to save sea otters were made and distributed, and thousands of publications outlining why the AEC tests weapons, how it ensures the safety of its testing, and other background information were distributed. The report also explains how it enlisted the support of a newspaper editor by associating him with the sea otter film and keeping in touch with him by letter and telephone, so that in the end he would accept and print AEC statements that the Alaskan public would not be endangered. The AEC also took space in two of the state's largest fairs and showed exhibits of archaeology on Amchitka and a pictorial history of the development of the site on Amchitka Island.

The report finally gives the details of a campaign conducted during the two weeks before the test took place and designed to offset public opposition in the press and on the radio and television. The most disturbing aspect of this phase in the activities, Gravel maintains, is the AEC's unwillingness to accept that its opponents were motivated by anything other than desire for publicity. For one thing, the report consistently terms the demonstrators in Canada as "hysterical", and castigates the press for giving extensive coverage to opponents of the AEC. Seemingly, anyone who wanted to get his name in the newspapers or on radio—regardless of expertise or qualifications—could do so by making a statement against the test, the AEC complains. The commission also resented its opponents bringing in seemingly irrelevant material, but nevertheless carried on distributing all kinds of information on other activities of the AEC to the state schools.

NEWS AND VIEWS

Does Molecular Shape Determine Odour Quality?

TRAINED odour judges are said to be able to distinguish as many as 15,000 different odours. One of the puzzling aspects of the sense of smell, however, is that there is little relation between chemical constitution and odour. Theories of olfaction are thus legion; all attempt to answer the central question as to what properties of the odourant molecules are responsible for the wide variety of odour quality.

M. M. Mozell holds the theory that the olfactory epithelium behaves towards odourants in a manner analogous to a gas chromatograph and that it is the temporal as well as the spatial aspects of the resulting neuronal response which gives rise to differences in odour quality (*J. Gen. Physiol.*, **56**, 46; 1970). J. T. Davies has the theory that the initial event in the olfactory stimulus is the puncture of a lipid membrane by the adsorption of an odourant molecule onto it (*J. Theoret. Biol.*, **8**, 1; 1965). He finds that when the molecular cross-sectional area of odourant molecules is plotted against their free energy of desorption from a lipid water interface, compounds of similar odour quality are found together in certain areas of the plot. These areas merge into each other indicating that there is a continuous spectrum of odour quality. Neither of these two theories requires the existence of specific receptor sites which respond to only certain types of odourant molecules.

Of the theories which start from the assumption that there are several different types of receptor sites, those of J. E. Amoore and of R. H. Wright at present excite the most interest. Wright starts with the idea, gleaned from Hainer's "information theory of olfaction", that there are about twenty-five different types of receptor. He considers that only the wide range of low energy molecular vibrations possible for different molecules could give sufficient variety to satisfy so many different receptors. The evidence for most of these vibrational modes is to be found in the far infrared spectra of the molecules and hence Wright has attempted to correlate odour quality with the frequencies of the peaks found in this spectral region. This correlation is not at all apparent on looking at the spectra and, even after Wright has picked out what he calls "osmically active bands", statistical tests are required to bring out a correlation.

From a study of the comparison between molecular shape and odour quality, Amoore came to the conclusion that there are seven primary odours, five of which are characterized by a particular molecular shape and each fits a corresponding receptor site with an appropriately shaped cavity. Other odours, supposedly more complex, are associated with molecules that fit into more than one of these hypothetical sites. This theory has met with some success in the prediction of the odour quality of new compounds. Amoore has since modified his ideas as to the number and types of primary odours and their corresponding sites, but still maintains that molecular shape is of major importance in determining odour quality.

On page 270 of this issue of *Nature*, Amoore makes an attempt to get a direct comparison between his theory and that of Wright. To do this he uses twenty-five compounds, the spectra of which have been measured by

Wright and which have odours similar to bitter almonds or cumin. In order to get a measure of odour quality he uses a panel of observers, selected for their olfactory acuity, to compare the odours with that of benzaldehyde. The numbers which he gives for odour similarity are essentially a rank order scaling. The procedure of comparison is analogous to asking persons of good aural discrimination to place a series of notes in their correct relationship to each other by comparing them with a standard note by ear only. This would probably result in the notes being placed in the correct sequence but the numbers assigned to the notes would show no more than a crude correspondence to their frequencies.

In the absence of any known odour property analogous to sound frequency, the odour similarity values are the only available parameters which can be used in testing theories of odour quality. Amoore finds a good correlation between his values and a computer generated index of similarity of molecular shape to that of benzaldehyde. He challenges Wright to show as good a correlation between these same odour similarity values and some measure of spectral similarity. This places Wright in the unenviable position that whereas failure to show a correlation will tend to discredit his theory, a correlation as good or better than Amoore's is unlikely to convert anybody not already committed to it.

Limits on Seyfert Galaxies

DECIDING whether the infrared flux from extragalactic objects such as galaxies and quasars varies significantly over short time scales has recently occupied much of the time of infrared astronomers. Significant flux variations on time scales of months or days would place quite severe constraints on the size of the infrared source in these objects. But infrared astronomy is a young science, and published evidence for time variations has so far been habitually followed—after a decent interval—by expressions of doubt that the evidence is convincing enough to be regarded as real.

Whether the same sequence of events will overtake the work reported by F. J. Low and G. H. Rieke of the University of Arizona on page 256 of this issue of *Nature* remains to be seen, but Low and Rieke find their evidence for a short-term variation in the infrared flux from NGC 1068 "compelling". Yet the fact that the change in the flux occurred at a time when there was a change in the instrumentation that was being used to monitor NGC 1068 is bound to raise a few eyebrows. But to be fair, Low and Rieke have done a convincing job in demonstrating that the low fluxes that have been recorded from NGC 1068 this year are likely to reflect an intrinsic change in the source, and are not an artefact introduced by the alterations to the equipment.

NGC 1068 is one of the Seyfert galaxies, which are characterized by small, bright nuclei and highly broadened emission lines setting them apart from other spiral galaxies. On average the Seyfert galaxies are no mean emitters of infrared radiation, but NGC 1068

surpasses any of them at these wavelengths—at the infrared wavelength of $22\ \mu\text{m}$ its flux density is thirty times that of the otherwise comparable Seyferts NGC 4151 and NGC 1275. But the placing of constraints on the model for the emission is of more general interest than its applicability to Seyfert, for the infrared emission from Seyfert galaxies is likely to arise by a similar mechanism to the unexplained infrared emission from quasars.

The infrared emission from extragalactic objects such as quasars and galaxies has previously been interpreted either in terms of synchrotron emissions from electrons, or by thermal emission from a shell of dust grains surrounding a compact source of energy. Neither view is entirely satisfactory, the synchrotron mechanism because an unusual spectrum of emission would be required which, although not totally impossible to obtain, would place difficult constraints on the behaviour of electrons in extragalactic objects which would be hard to fulfil. On the other hand, the dust model is usually regarded suspiciously because of the fear that time variations of the infrared emission would one day show it to be untenable, even though the observational evidence has not been convincing.

In the dust model, a central source in the nucleus of the Seyfert galaxies is taken to emit energy in the form of ultraviolet radiation, or possibly as particle radiation, which is thermalized by the shell of dust grains around the central source. The grains re-emit the energy, chiefly in the infrared. But as Low and Rieke point out in their article, and as others have, of course, done before them, short-term variability of the emission will place constraints on the size of the dust shell, the constraints becoming more severe as the time scales are reduced from months to days.

According to Low and Rieke, the observations reported this week are consistent with an optically thick shell of dust having a diameter of the order of 3 parsec (about ten light years) containing dust at 400 K, or an optically thin shell having a diameter less than about 0.3 parsec at a temperature greater than 1,200 K. So, in addition to verifying the variability by continuing the observations on NGC 1068 and other Seyferts, astronomers will want to discuss whether these size and temperature constraints can be met.

DEVELOPMENTAL BIOLOGY

Embryologists Meet in Glasgow

from a Correspondent

THE tenth international conference on embryology, which took place in Glasgow on August 30 to September 3, was well run, with only three principal speakers in each session. Each speaker therefore had enough time to develop an argument and could speak to a relatively fresh and attentive audience. The sessions included oogenesis, genetic interactions in development, the control of protein synthesis, the development of pattern and neuroembryology. There was also a film session, several meetings of discussion groups and about ninety demonstrations.

Dr. F. H. Ruddle (Yale University) showed that the assignment of linkage groups to particular human chromosomes was proceeding rapidly. In his work he uses the technique of intra-specific somatic cell hybridization and half-selection to produce hybrids in which the expression of particular differentiated traits can be examined. For example, in a hybrid derived from a mouse renal adenocarcinoma (RAG) and a human fibrocyte (Wi 38), cells were either deficient in esterase-2 activity, which is characteristic of the human parent, or possessed it, which is characteristic of the mouse parent. On prolonged cloning, the ES-2⁻ strain occasionally reverted to ES-2⁺, but ES-2⁺ never reverted. Ruddle concluded that it was possible that the expression of the mouse phenotype could be controlled by the presence or absence of a human chromosome, either C8 or C9.

Dr E. Sidebottom (University of Oxford) uses hybrids between chicken erythrocytes, which do not normally show nuclear activity, and HeLa cells in combination with antisera to human nucleoli, nucleoplasm and cytoplasm. He was able to show a good correlation between the renewal of DNA and RNA synthesis by the reactivated chick nucleus and the appearance of chick antigens on the cell surface shortly after the reappearance of chick nucleoli. He found that although human nuclear material occasionally appeared in chick nuclei, human cytoplasmic material never did. Similarly, in mouse-human hybrids he demonstrated the synthesis of a human enzyme under the control of mouse nuclear material.

Dr P. J. Bryant (University of California, Irvine) discussed the properties of *Drosophila* imaginal disks—the segmented larval tissues from which adult structure develops. Each disk is a more or less tubular structure which is folded and attached by a stalk to the epidermis. After the last moult the disks evert, unfold and differentiate to produce a segmental structure such as an eye or a limb. He used genetic mosaics, made by irradiating flies heterozygous for "yellow" and "singe" (pale cuticle and bent bristles). This leads to somatic crossing-over in cells of the imaginal disks, some of them becoming homozygous by recombination so that the characters are expressed. Adults therefore have patches of pale cuticle and

Selection in Quasar Redshift Measurements

IN the flurry of publications concerning the observed clumping of quasar emission line redshifts at certain values, very little attention has been given to the problem of selection effects. Nobody would argue against the suggestion that the observations favour some values of redshift (z). But it is a different matter when the question of periodicities in the favoured values of z are considered, and this is an opportune time for R. C. Roeder's analysis in next Monday's *Nature Physical Science*.

The most well known and least questionable region of favoured redshifts lies around $z=1.95$. Various authors have claimed that other peaks in the histogram of quasar redshifts can be related to this peak and one another, with a "periodicity" in z of 0.06. Other workers have argued with equal conviction that the statistics are inadequate to support such a claim, and the possibility of selection effects has also been

raised, although not usually in any explicit form. Roeder's work now suggests that in some objects there are simply more spectral lines to measure—in a few cases at the other extreme redshifts have been claimed on the basis of the "identification" of only one line in the spectrum of a quasar.

The correlations observed by Roeder are statistically significant, and of course if there are more lines to measure at some redshifts then it is easy to understand why more quasars have had their redshifts measured at these values. It seems that the "ease of measurement effect" may be related to the movement of spectral features across the optical window as z increases. Most notably, the deficiency of redshifts measured at $z=1.25$ could be attributed to the shift of Mg II out of the window, and the rise at $z=1.8$, peaking around $z=1.95$, could be attributed to the shifting of Lyman α into the window.

epidermis bearing "sing" bristles on a normal background. Each pattern presumably derives from a single clone, because its outline is entire and cells are never individually determined.

From experiments in which the legs were transected, Dr Bryant showed that there was good evidence for an axial developmental gradient which could partially regulate. The upper part of the limb tends to develop normally, the lower half to duplicate with mirror symmetry. This is as if a single gradient is halved: the low end of the top half relaxes towards zero, presumably because of diffusion of the substance in the gradient, the high end of the bottom half doing the same. Some positional values for specifying the limb structure thus only occur in the original top portion of the gradient, whereas those in the relaxed lower portion are duplicated. (Schubiger has reported similar results and it is interesting that there is a lethal mutant with giant disks in which structures tend to duplicate, possibly because the disk becomes too large to be covered by a single gradient.)

The most significant result of the conference was the rejection, by Dr R. M. Gaze (National Institute for Medical Research, Mill Hill) and others, of the ideas of neuronal specificity and modulation. By neuronal specificity is meant the possession of specific chemical labels by neurones and their processes. Correct recognition of such labels is supposed to be instrumental in the formation of ordered connexions between, for example, the optic nerve and the rectum in amphibians. Remodulation is the relabelling of central neurones as a function of their peripheral connexions. Neuronal specification came to be discussed because in those situations for which it has been theoretically required it now seems that there is relatively little specificity involved.

Furthermore, connexion patterns can regulate after damage in such a way that axons become inputs to neurones that they would not originally have contacted. For example, if half the optic tectum is removed and the optic nerve regenerates, the system regulates to preserve a retinotopic projection. As Gaze has shown, normal retinal growth is by division of cells in a ring at the ciliary margin. It is likely that each cell divides to leave a central daughter which will differentiate and a peripheral stem cell which will divide again. The axes whereby the position of retinal ganglion cells are specified are determined when the retina is only twenty cells in diameter, so that the specificity involved might not be very great—connexions subsequent to those of the first disk of cells could be made in the order in which ganglion cells mature and send out their axons. The tectum grows

from a single line of dividing cells, however, so that the actual geometry of connexion formation is still difficult to envisage.

Most experiments in which remodulation is supposed to occur can apparently be reinterpreted as if random neuromuscular connexions are made after a motor nerve is cut. The patterns of movement of a reinnervated limb are in fact so limited that they cannot afford evidence for either specific reconnection in detail, or for remodulation. Indeed, Dr L. Marotti (Monash University) showed that in the goldfish the inferior and superior oblique eye muscles can both be reinnervated by the nerve supplying the inferior oblique. Apparently, however, even though "incorrect" synapses are formed they are not functional under physiological conditions, though she has not yet checked to see whether they are electrically excitable.

RNA SYNTHESIS

Messenger Mathematics

from our Cell Biology Correspondent

THERE are obviously two potential ways in which a bacterial cell might increase or cut back its synthesis of RNA and protein in response to changes in its environment which either increase or decrease the growth rate. The rate of elongation of RNA and polypeptide chains could be altered, or the number of new chains initiated could be regulated while the rate of elongation of chains once initiated remained constant. During the past several years at least half a dozen groups have grappled with this question and the majority opinion is that the rates of elongation remain more or less constant in cells which are growing at rates differing by up to six-fold. The implication of these results is, of course, that regulation occurs at the initiation step. But one or two dissenters have reported evidence which suggests that rates of elongation may vary and this evidence, together with the fact that in most previous experiments the range of growth rates of the cells was restricted, prompted Coffman, Norris and Koch (*J. Mol. Biol.*, **60**, 1; 1971) to measure the kinetics of appearance of β -galactosidase after the addition of inducer to cultures of *Escherichia coli* with doubling times ranging from 50 minutes to 24 hours.

Exploiting the sophisticated techniques now available for measuring the early kinetics of enzyme induction and a sensitive fluorometric assay for β -galactosidase, Coffman *et al.* found that the delay time—the interval between the addition of inducer and first appearance of detectable β -galacto-

sidase activity—was 95 ± 5 seconds in cells with doubling times ranging from 50 minutes to 12.5 hours. And even in cultures doubling extremely slowly (doubling time 24 hours), the delay time was a mere 150 seconds and in this extreme case the binding and penetration of inducer may be retarded.

In short these experiments confirm that over a thirteen-fold range of growth rate the elongation of RNA and polypeptide chains in *E. coli* is invariant. Furthermore, the average lag between adding inducer and the achievement of a steady-state of β -galactosidase activity does not vary significantly with changes in the growth rate; this finding presumably means that messengers survive for about as long in cells doubling every 50 minutes as in cells doubling every 12.5 hours.

In an appendix Koch presents an algebraic analysis of the induction curves for β -galactosidase, obtained when cells are either pulsed with inducer or allowed to grow in the continued presence of inducer, in an attempt to decide between the various models proposed to explain the decay of a messenger RNA. He claims that the results of experiments in which inducer is added and not removed from the medium exclude models which envisage that each messenger is read for a fixed number of times before it is degraded. The data are, however, compatible with a model which envisages that messenger decay is random with respect to time and place, or a model which envisages that there is a constant probability that a "killer" ribosome will initiate translation and inactivate the messenger as a result.

While Coffman and his colleagues have been measuring the kinetics of expression of the *lac* operon—and the objection that their data are relevant only to inducible operons cannot as yet be ruled out—Eron, Morse, Reznikoff and Beckwith (*ibid.*, **60**, 203; 1971) have been looking at the transcription of the *lac* genes in a strain of *E. coli* which because of a deletion has the structural genes of the *lac* operon fused to the *trp* operon. In this strain the expression of the *lac* genes is regulated by the controlling elements of the *trp* operon, and Eron *et al.* have tested to see if a *trp/lac* fused messenger RNA is ever made. They find that some 4–10 per cent of the messengers made from these two fused operons are indeed covalently linked. They conclude that an RNA polymerase can initiate at the *trp* promoter and read through the *trp* and into the *lac* operon. But why is the amount of fused messenger so small? Either read through is rare, or it is the rule, but for one reason or another the fused messenger is broken during isolation or its 5' end is rapidly degraded *in vivo*.

REEFS

Old and Modern

from a Correspondent

UNDER the auspices of the Palaeontological Society, a well attended meeting on the palaeoecology of reefs was held at the University of Edinburgh on September 9, being one of the events held to celebrate the centenary of the Department of Geology.

Drs D. J. Bellamy (University of Durham) and E. Drew (University of St Andrews) described their work on the zonation of hermatypic corals off the reef front at Aldabra including reference to a major system of channels which are explicable only on the basis that they represent the effect of erosion during aerial exposure of a preglacial formation. Dr P. Garrett (University of Leeds) transferred attention to the reefs of Bermuda and emphasized the complexity of a reef mass formed primarily by coral skeletons, the overlapping growth of which produces internal cavities encrusted with algal and animal growths.

Dr J. Taylor (British Museum (Natural History)) drew attention to the high diversity of shallow water macro-invertebrates in coral reef areas. Reefs present a unique diversity of habitats—a consequence of the resistance their formation presents to the forces of the sea—ranging from exposed reef faces to sheltered sandy and mangrove areas. Computers are now used to obtain objective comparisons of habitats of molluscan species.

Dr C. J. R. Braithwaite (University of Dundee) was concerned with the need for criteria for the recognition of fossil reefs and drew attention to those features of modern reefs which are particularly significant, notably the platform with its superimposed organisms. The fact that recent reefs have grown upon Pleistocene formations presents a major difficulty when comparing these with fossil reefs. Dr B. R. Rosen (University of Newcastle upon Tyne) considered ecological stratification in coralline environments in terms of hard and soft non-living substrates for life on which corals are very differently adapted.

Dr T. P. Scoffin (University of Edinburgh) described conditions of growth in the Wenlock reefs where the most vigorous growth occurs on what was the seaward fringe; colonies are preserved in the condition of growth indicating calm conditions. Dr C. T. Scrutton (University of Newcastle upon Tyne) spoke about the effect of the environment on fossil colonies and indicated that rugose corals were generally less well adapted for life in rough, "high energy", environments than were the probably poriferan stromatoporoids. Drs F. M. Broadhurst and I. M.

Simpson followed by Dr J. Miller (all University of Manchester) were concerned with descriptions of limestones of coral reef origin. In the Castleton area of Derbyshire the limestones show marked faunal changes marginally, and the limestones present in knolls around the Clitheroe area, described by Dr Muller, are associated with dense crinoid thickets. Dr D. B. Smith (Institute of Geological Sciences, Leeds) dealt with the well known Upper Permian barrier reefs of the Guadalupe Mountains and compared these with smaller but similar reefs in Durham.

The meeting concluded with a lively discussion on many of the matters raised by the various speakers; a satisfactory definition of the term "reef" proved hard to find. Finally, Sir Maurice Yonge drew attention to some recent work on living corals which is providing some interpretation of the factors controlling the distribution and ecology of reef-building (hermatypic) corals.

FLUID MECHANICS

Pneumatic Conveying

from a Correspondent

THE behaviour of gas-suspended solid particles flowing in pipes is of much interest in modern powder processing industries. For many years this topic of research has also been a satisfying bone for many academic fluid dynamacists to chew on. For the first time a conference has been devoted entirely to pneumatic conveying. The meeting was held at Churchill College, Cambridge, from September 6 to 8, and was sponsored by the British Hydro-mechanics Research Association in conjunction with the City University of London.

On the applications side, systems for conveying some rather troublesome materials were described; asbestos slurry was the theme of a contribution by Dr D. C.-H. Cheng *et al.* (Warren Spring Laboratory, Stevenage); on-site con-

Messenger Transport *in vitro*

MESSENGER RNAs transcribed in the nuclei of eukaryotic cells have to be transported to the cytoplasm to be translated and there is increasing circumstantial evidence that this transport process is an important regulatory step in the expression of genes. The process is, however, extremely difficult to analyse not least because of the heterogeneity of nuclear RNAs and the biochemical complexity of cell cytoplasm and these factors prompted Raskas, as he describes in next Wednesday's *Nature New Biology*, to attempt to set up an *in vitro* system for the analysis of RNA transport across the nuclear envelope. He elected to use cultivated human KB cells infected with adenovirus type 2 as a starting material because some 18 hours after infection most RNA being made in the cell nucleus is adenovirus specific, and he now claims that, in the presence of an ATP generating system, nuclei isolated from such cells release RNA by a mechanism similar to that occurring *in vivo*.

The release of RNA by isolated nuclei is strictly dependent upon a continuous supply of ATP, without which less than 10 per cent of the RNA leaves the nuclei, and once released the RNA remains precipitable by TCA and presumably, therefore, undegraded. Analysis of the released RNA by centrifugation in sucrose gradients reveals two broad peaks, one sedimenting at about 35S and the other at about 10S. When the salt concentration is increased, the 10S RNA behaves as before but the 35S peak disappears because, Raskas suggests, it consists of

ribonucleoprotein complexes which are dissociated at high salt concentrations. The buoyant density of this material is consistent with this suggestion.

Both 10S and 35S components contain viral RNA which will hybridize with adenovirus DNA, and an analysis of the total RNA released by the isolated nuclei in 20 min on polyacrylamide gels reveals RNA molecules long enough to be adenovirus messengers, in addition to low molecular weight RNAs, and the 6S RNA synthesized at late times during adenovirus 2 infections. Raskas suggests that the release of these RNAs *in vitro* is relevant to transport *in vivo* for several reasons. First, the release is specific; significant amounts of nuclear protein and DNA are not released. Second, the size of the released RNAs suggests that a maturation by cleavage goes on inside the isolated nuclei as it does *in vivo*. Third, the release *in vitro* cannot, of course, depend on the synthesis of new ribosomes and studies of intact infected cells indicate that movement of adenovirus RNA into the cytoplasm does not depend on ribosome synthesis. Finally, much of the adenovirus RNA released *in vitro* is in the form of a ribonucleoprotein and the same seems to be true of transport *in vivo*.

There is therefore every reason to believe that further analysis of this *in vitro* system will throw light on the process of RNA transport and, as Raskas comments, one of the first things to do is to analyse the protein components of the 35S ribonucleoprotein material to see if only a few, specific "transport" proteins are present.

crete distribution was described by Dr R. H. Jones (University of Nottingham); even proposals for a domestic refuse-disposal system were outlined by Dr J. A. Hayden (Hayden, Harding and Buchanan Inc., USA) and Dr A. W. Breidenbach (Environmental Protection Agency, USA).

On more fundamental issues the meeting was rather disappointing in that few contributions added much to present understanding of basic flow mechanics. The influence of dense, locally-concentrated particle clouds on basic fluid mechanics was scarcely mentioned. Furthermore, agglomeration and other multi-particle collision effects received rather scant attention, as was pointed out by Dr C. J. Stairmand (ICI Ltd, Teeside) in the discussion.

More definite advances were reported in the contributions describing experimental work. In particular, a more interesting statistical approach to particle continuum mechanics was evident in the work of Dr M. S. Beck *et al.* (University of Bradford) and Drs R. G. Boothroyd and P. R. Arundel (University of Birmingham). Drs J. Jokati and Y. Tomita (Kyushu Institute of Technology, Japan) described another example of more useful presentation of experimental data: they gave probability distributions of particle velocities at different points in a pipe. This break from earlier research, in which the interest has tended to remain in time-averaged fluid-dynamic parameters, is a most welcome and necessary development. Professor S. L. Soo (University of Illinois) twice expressed the opinion that the facility with which experimenters can examine the motion of individual particles should not be an excuse for ignoring the continuum viewpoint of the solid phase. To illustrate his point he reminded his audience that the motion of molecules is rather unimportant in one-phase fluid dynamics.

HISTORY OF SCIENCE

Kepler Honoured

from a Correspondent

THE thirteenth international congress of the history of science, held at Moscow State University from August 18 to 24, and followed by an international symposium at the Leningrad Planetarium from August 26–28, held to celebrate the four hundredth century of Kepler's birth, provided delegates from many countries with some chance of meeting Soviet scholars and seeing the fine collections in the host country.

Academician B. M. Kedrov opened the congress with an address on the history of science and principles of its research, in which he emphasized the importance of linking together past,

present and future in the development of science. Colloquia topics (for which full simultaneous translation facilities were available) included the Rutherford centenary and seventy-five years of radioactivity studies (see *Nature*, **233**, 167; 1971); the evolution of classical mechanics; Mediaeval science: East and West relationships; ways of Development of Functional Analysis; and new technology in the developing countries from the eighteenth to twentieth centuries.

The general standard of contributions was high, but discussion time was, as

so often is the case, somewhat restricted. The colloquium on the teaching of history of science was rather disappointing and uncoordinated; the Royal Society *Report on the Teaching of the History of Science, Medicine and Technology in Universities and Technical Colleges in the United Kingdom 1971* was presented, but was felt by many participants to be an uneven document.

The sections were less formally organized and did not entirely avoid language problems. It was felt by some participants that some of the "questions" asked were more of the nature

More about Interstellar Grains

IN spite of increasing efforts to identify the composition of interstellar grains, conclusive results remain elusive. Observations in the past were restricted to visible wavelengths where the shape of the interstellar extinction curve is fairly uniform throughout the galaxy and almost featureless, apart from a slope discontinuity at 4300 Å and several diffuse bands. Now that ultraviolet and infrared astronomy have widened the scope of investigation, the accumulating evidence points to large differences in the general trends of extinction from region to region. There are also several observed spectral features to be explained and the discovery of more can be anticipated. The conspicuous 2200 Å peak has been frequently attributed to graphite, although theoretical calculations tend to predict too broad a feature. Silicates and hydrocarbons have recently been proposed as alternatives and no doubt others exist.

In the infrared, much of the activity is concerned with emission from circumstellar shells (for example, Gillett *et al.*, *Astrophys. J.*, **164**, 83; 1971). The characteristic 10 μm excess in luminous M stars is suggestive of silicates and a limited amount of 20 μm photometry has supported this. Spectral features observed in absorption may, however, be more representative of intrastellar dust, because circumstellar grains are supposed to have been produced locally in the stellar atmosphere. Several unsuccessful searches for the 3 μm ice band indicate that most interstellar grains contain little ice, at least in the proposed form of H₂O ice (Knacke *et al.*, *Astrophys. J.*, **158**, 151; 1969); the 3 μm absorption in NML Cygnus, however, remains unexplained. Medium bandwidth observations of the bright infrared continuum of the galactic centre (20 to 30 magnitudes of visual extinction) combined with other observations have been interpreted by Hackwell *et al.* (*Nature*, **227**, 822; 1970) as showing absorption near 10 μm and 20 μm due to metallic silicates.

These results must be regarded as tentative because of the considerable difficulty in establishing the true continuum against which absorption or emission is to be distinguished. Observations of three B supergiants were inconclusive because of the much lower amount of extinction, and an earlier measurement of 10 μm absorption in the M2 Ib star 119 Tau was not confirmed.

Following up these observations W. A. Stein and F. C. Gillett report in next Monday's *Nature Physical Science* a search for 11 μm absorption in the highly reddened B8 Ia star IV Cyg No. 12. Although most of the ten magnitudes of visual extinction probably arise from dust localized in the OB association, possibly even in the immediate vicinity of the star (Reddish *et al.*, *Pub. Roy. Obs. Edinburgh*, **5**, 111; 1967), evidence such as the shape of the extinction curve, the amount of polarization and the strength of the diffuse 4430 Å band suggest a similarity to interstellar grains. The continuum beyond 5 μm seems well established as the Rayleigh-Jeans portion of the stellar spectrum; there is no circumstellar emission out to 11 μm although there could be at longer wavelengths. More important, no evidence for absorption was found, the upper limit to the 11 μm optical depth being $\tau=0.1$. This limit is at least a factor of 2 to 3 lower than predicted by scaling the observations of Hackwell *et al.* ($\tau \approx 0.7$) but abundance variations of this size might be expected.

Stein and Gillett also estimate that less than a third of the mass of the grains can be metallic silicates. With the improved infrared optical constants reported by Nandy and Kelly (XVII Liège Symposium, June 1971) this estimate could be refined and extended to several other minerals. Clearly, many more observations with as high resolution as possible and at as many wavelengths as possible will be required before a definitive identification of the interstellar grain materials is finally achieved.

of doctrinal filibusters than requests for clarification or information. Questions such as methodology, the organization of research and the development of technologies, are obviously more open to controversial social and political interpretations than is, say, mediaeval mathematics; the time range of sectional topics from ancient science, medicine and technology to the development of transport, electronics, constructional engineering and aircraft, rockets and space science, provided most speakers with a forum.

The Kepler symposium at Leningrad was a much smaller and more homogeneous affair. The location was very appropriate because most of Kepler's surviving manuscripts and related papers are in Leningrad, and many of these were included in a small and interesting exhibition of works on and by Kepler. There was also an opportunity to visit the nearby Pulkova observatory, rebuilt following its wartime destruction, and see and discuss many aspects of modern astronomy. Academician Mikhailov opened the proceedings, and was followed by Dr W. Gerlach (Munich) who spoke on Kepler's life and work. Subsequent contributions (not all presented in person) dealt with Kepler's contributions to celestial mechanics, optics, the development of mathematics, gravitation theory, comets, the supernova of 1604, structural crystallography, the origin and derivation of the laws of planetary motion and his neopythagoreanism. There were also contributions on Kepler's connexions with Galileo, his time in Prague, his astronomical instruments, his theory of hypotheses, the Kepler manuscripts in the Soviet Union, as well as his relationship to the interest of modern physicists. Although no particularly outstanding new information or insight into Kepler's work was presented, it was clear that his scientific and methodological achievements continue to be of interest and even inspiration to many historians of science, quite apart, of course, from the historical problems involved.

VIRAL INFECTIONS

Green Monkey Disease

from our Medical Virology Correspondent

IN August and September 1967, an outbreak of a severe haemorrhagic fever occurred among monkey handlers and laboratory workers in Marburg, Frankfurt and Belgrade. Thirty-one cases of severe illness with seven deaths were traced to direct contact with the tissues or tissue culture of a batch of African green monkeys (*Cercopithecus aethiops*), which had been trapped in Uganda. Several secondary cases occurred in hos-

pital personnel by contact with the blood of patients and one further case was apparently transmitted by sexual intercourse. Many of the features of the illness, including liver and renal damage, a haemorrhagic diathesis and the association with monkeys originating from Uganda, suggested initially that the infection was caused by yellow fever virus or some other arbovirus. It soon became apparent, however, that this was a previously unrecognized disease caused by an infectious agent probably new to medical science. Today, four years after the infection, the origin of the disease remains obscure.

Studies in several laboratories established that guinea pigs, Rhesus (*Macaca mulatta*) and vervet monkeys could be infected and the agent was subsequently isolated in tissue culture. The structure of the agent, which was named the Marburg virus, is that of an elongated sinuous organism 800–1,000 nm in length but generally curled in the form of a figure 6 or a horseshoe. Single organisms longer than 2 μ were occasionally found in the blood. A periodic cross-striation of 5 nm along the surface and an inner structure of about 65 nm in diameter with a central axis was also described. It was suggested that this agent was related morphologically to the Rhabdovirus family.

The possible origin of the infection has been investigated intensively since the outbreak. The monkeys were sent from Uganda at the time when there was considerable disruption of air traffic during the Six Days war. The monkeys were held overnight in London in a room containing many other animal species including birds and reptiles. A disturbing report from the United States based on serological study of complement-fixing antibodies to the Marburg agent revealed a high prevalence of antibody in many African monkeys. Strickland-Cholmley and Malherbe (*Lancet*, i, 476; 1970) pointed out, however, that the antigen used in the United States was not fully specific and that it probably contained a second non-specific component for the Marburg agent. W. Slencza and his associates (*Amer. J. Epidemiol.*, 93, 496; 1971) were also unable, after extensive studies, to find evidence of infection with the Marburg agent (based on antibody results) in free-living monkeys.

The origin of the infection is thus still unknown and it must therefore be assumed that infection of man may recur. Furthermore, infection may come next time from other species of animals because it is now clear that vervet monkeys are not the maintenance hosts of this bizarre agent and it cannot be predicted where it may next occur.

Protein Kinase in Tumour Viruses

IN *Nature New Biology* next week, Strand and August add a protein kinase to the list of enzymes, which includes reverse transcriptase, ligase and nucleases, that have been detected in RNA tumour viruses. The kinase, which is present in mouse leukaemia virus, avian myeloblastosis virus and the non-oncogenic vesicular stomatitis virus, catalyses the phosphorylation of several virion proteins. Its presence in these particles suggests that modification of proteins by phosphorylation may play a part in the interaction between an infecting enveloped virus and its host cell.

The kinase was detected by simply incubating mouse leukaemia virus particles with (γ - ^{32}P) ATP and assaying for the incorporation of ^{32}P into virion proteins. Further experiments established that the kinase seems to be an integral part of the virion and is not adventitious; the phosphorylation reaction is stimulated by added protamine, depends on the presence of metal ions, is inhibited by caffeine and is enhanced by first disrupting the virions with detergent. Cyclic AMP, which stimulates by some two to three-fold the activity of other cellular kinase, and by this route exerts at least some of its regula-

tory effects, stimulates the phosphorylation of leukaemia virus proteins by only 25 to 50 per cent in the absence of protamine and in its presence stimulation is even less marked. As Strand and August remark, this lack of response of the virion kinase to cyclic AMP may mean either that the enzyme already has bound to it this nucleotide or it may lack the regulatory subunit of kinases which binds cyclic AMP.

The virion kinase will phosphorylate exogenous substrates including lysine and arginine rich histones, and a protein of *Escherichia coli* which is one of the two host factors required for the replication *in vitro* of QB RNA and which closely resembles histones. In the absence of added substrates the kinase phosphorylates at least ten different proteins or polypeptides in leukaemia virus particles including the internally located interspecies specific antigen (gs 3) and the two group or species specific antigenic determinants. Chemical tests indicate that serine and threonine residues are phosphorylated. Strand and August have not, however, been able to determine whether or not the glycoproteins in the envelopes of the virions, including the sub-group antigens, are phosphorylated.

NUCLEIC ACIDS

Collapsible DNA

from our Molecular Biology Correspondent

WITH a metre or so length of DNA in each of one's cell, it is fitting that one should from time to time ponder the question of how this relates to the well-known hydrodynamic rigidity of the thread of life, once this is out of the living cell and in the ultracentrifuge cell, where in the view of many people it properly belongs. Its properties, one might hazard, are modified by the basic proteins in which, in higher organisms, it is packaged. In other surroundings, such as phage heads, such a facile evasion will not do, and this is the starting point of Lerman's efforts (*Proc. US Nat. Acad. Sci.*, **68**, 1886; 1971) to find conditions in which native DNA will pack into a compact form, as it does in the phage.

In dilute salt solutions DNA is a rather stiff "worm-like coil". In the presence of high concentrations of neutral, unreactive polymers, Lerman reasoned, an effective phase separation, such as occurs macroscopically in mixtures of mutually repelling polymer species in solution, could be achieved by a collapse of this extended structure into a compact domain. Moreover, if the intermolecular forces in such a system are indeed predominantly repulsive, one would not expect that this would have to be accompanied by denaturation of the double helix.

The argument was evidently sound, for in the presence of the neutral polymer, polyethylene oxide ('Carbowax'), and at a sufficient salt concentration, presumably to minimize internal repulsions, a dramatic collapse of the DNA structure occurs. T4 phage DNA, which normally sediments at about 80S, now forms a broad band in a sucrose-D₂O gradient, centred at about 300S, and extending to 500-600S. With T7 DNA the results are qualitatively similar: from a normal sedimentation rate of 62S, it changes to give a peak at 200-250S, trailing out to 400S. The transition to the compact state occurs within a critical range of neutral polymer concentration and of sodium chloride (between 0.2 and 0.3 M). The polyethylene oxide itself undergoes no detectable change, as judged by its viscosity, in this ionic strength range, and moreover another neutral polymer, polyvinylpyrrolidone, will serve just as well. A striking demonstration of the existence of a threshold polymer concentration was achieved by sedimenting the DNA through a sucrodensity gradient containing an inverse concentration gradient of polyethylene oxide. On encountering a low polymer concentration the DNA reverts to the slowly sedimenting form, so that the

back of the sedimenting zone catches up the front, and practically stops.

Examination of the buoyant density of the compact DNA after sedimentation excludes any question of denaturation. The sedimentation patterns are independent of DNA concentration, so that one may take it that the rapidly sedimenting form is not aggregated material. The high partial specific volume of the neutral polymer means that the formation of a complex with DNA, even with a large excess of polymer, would make little impression on the sedimentation coefficient of the DNA. Moreover, a similar effect is exerted by polyacrylic acid, which it would be hard to envisage forming a complex with DNA. It thus seems justified to infer that a true compact form is indeed generated. One may calculate sedimentation coefficients of the DNA molecule in various forms,

and one may with reasonable certainty exclude supercoiling, which would change the sedimentation rate far too little, and a range of other possibilities. A compact sphere would sediment too rapidly, but an ellipsoid of axial ratio about 7:1 would give a result of the right order, and so would a platelet made up of double-helical stretches of DNA some 160 Å long folded concentrically on each other.

So far as theory for phase relations can be applied, it is by no means inconsistent with a microscopic phase separation in the DNA-polymer system. Lerman suggests that the intracellular environment, pullulating as it is with other macromolecules, may be not unlike his model system. The implications of the ready transition from a stiff to a highly folded form in relation to the structure of the double helix will have to be carefully considered.

Reverse Transcriptase Initiates on RNA Primer

In next Wednesday's *Nature New Biology*, Baltimore and four of his colleagues report that reverse transcriptase requires an RNA primer to initiate the synthesis of a DNA chain, which is covalently bound to the primer RNA. They describe a series of experiments which prove beyond doubt that the enzyme initiates DNA synthesis by adding a deoxynucleotide to the 3' hydroxyl group of the primer. In short, the mechanism of DNA synthesis by reverse transcriptase seems to be essentially identical to that of the Kornberg DNA polymerase I of *Escherichia coli*, except the reverse transcriptase can use an RNA primer and an RNA template. The similarities between the two enzymes are indeed so great that Verma *et al.* even suggest that an RNA molecule might serve as the initiator or primer for DNA synthesis in *E. coli* by enzymes such as DNA polymerase I.

The first real clue that reverse transcriptase might require a primer RNA came from experiments with exogenous templates. Baltimore and Smoler found that although the enzyme often uses exogenous templates more efficiently than the endogenous tumour virus genome, it does so only when a primer, which can be as short as a tetranucleotide, is present; the product made comprises the new DNA chain physically attached to the primer. Having made these observations Baltimore's group asked the obvious and important question—does the enzyme when it transcribes the endogenous tumour virus genome require a primer and if so what is the primer?

They first noticed that the 20 minute product of the endogenous polymerase

reaction with murine leukaemia virus particles was a single stranded DNA with a density slightly greater than expected. By treating this DNA with ribonuclease or alkali its density was reduced to that of single stranded DNA. Turning to avian myeloblastosis virus particles, which can be had in large amounts, they analysed the product DNA made after 2 to 30 minutes by the endogenous reaction. As the reaction proceeds the density of the DNA product shifts from that of a 1:1 RNA:DNA duplex to close to that of single stranded DNA.

After a series of treatments which hydrolyse RNA all the reaction products assumed the density of single stranded DNA. Using purified avian myeloblastosis virus enzyme and isolated avian myeloblastosis virus RNA instead of the virus particles, they obtained essentially identical results. And experiments with the inhibitor of reverse transcriptase, dideoxythymidine triphosphate, which have yet to be published by Baltimore's group apparently confirm that DNA synthesis is initiated by the attachment of the first deoxynucleotide to the 3' hydroxyl group of the primer.

But what acts as primer in the endogenous reaction? It could of course be that the 3' end of the 60-70S viral RNA genome forms a hair pin bend; alternatively small RNA molecules might associate with the 60-70S RNA. Small RNAs, including transfer RNAs, are present in tumour virus particles and Baltimore's group favour the idea that these act as primers, not least because many DNA molecules are made per viral genome.

ACOUSTICS

Hungarian Meeting

from a Correspondent

THE seventh international congress on acoustics, held at Budapest from August 18 to 26 under the presidency of Professor Tarnöczy of Hungary, was the first occasion on which the meeting had been held in a country of the Eastern block and it is noteworthy that just over 40 per cent of the 1,200 delegates came from these countries. There were also 300 guest members and the size of the gathering emphasizes the growing importance of the subject.

The revived interest in models was evident in the appearance of a few contributions on the subject such as those on the use of a model to evaluate various possible changes to a studio by Dr A. N. Burd (BBC Research Laboratory), and on diffusion and absorption of sound in a model reverberation room by the fathers of modern acoustics, Drs V. O. Knudsen and L. P. Delsasso (University of California, Los Angeles). It is sad to report that Delsasso died suddenly when travelling to the congress.

A balanced selection of topics over the wide field of acoustics had been chosen for the invited fourteen plenary one-hour lecturers. Unfortunately for many listeners nearly half of these were in French or German. Dr J. L. Flanagan (Bell Laboratories, Murray Hill) gave an excellent lecture with demonstrations of advances in selected areas of speech communication. The advances in research in this field are largely due to the expanding and the in-depth understanding of the theory of sampled-data systems and also to the wide adaptability of the moderately sized, fast digital computers. The transmission of speech in digital form, by contrast with analogue transmission, can be made effectively independent of distance as regards quality of reception, and a variety of signals—for example, voice, telegraph and the like—may be interleaved without special consideration. Digital encoding methods for speech are very varied and source coding leading to lower bit rates has resulted in an increased study of speech synthesis. Synthetic speech provides storage economies and vocabulary flexibility which promotes different possibilities in new automatic information services, and every push-button telephone becomes a potential computed terminal. For use in computer answer-back the synthesis of formant data or of printed text both seem attractive. With printed text, the machine stores a pronouncing dictionary which includes a phonemic transcription of each word, word stress and rudimentary syntax information. It generates connected speech solely from a knowledge of the linguistic rules of

the language and the dynamic acoustic properties of the human vocal apparatus.

The frequency range of acoustical investigations has been extended considerably upwards in recent years and, as was mentioned by Dr E. G. S. Paige (Royal Radar Establishment, Malvern), mechanical surface waves can now be generated up to 10 GHz. Mechanical waves of these frequencies (up to 100 GHz) are often termed hypersonic and are being used in physical investigations of the condensed state of matter involving their interaction with electromagnetic, spin and acoustic waves (Dr J. P. Joffrin, University of Paris). At the other extremity of the frequency range (Dr R. W. B. Stephens, Imperial and Chelsea Colleges, London), the wavelength of the sound radiation becomes comparable with the natural frequency of various parts of the human

body, and unpleasant, if not dangerous, effects can arise in consequence. There is a real need for more planned investigations in this frequency region.

The growing field of ultrasonic applications to medicine was made evident by the appearance of several contributions from workers in various countries. Dr J. C. Somer (TNO, Utrecht) described his development of an array of electro-acoustic transducers which comprises twenty-one elements, spaced a half wavelength apart and suitably phased. This technique has been adopted for electronic sector scanning, and provides two-dimensional scanning with a substantially stationary probe. It has particular application to diagnosis of the brain where mechanical scanning has not proved very successful chiefly because of the high absorption of the skull.

Bacterial Purple

NEXT Wednesday's *Nature New Biology* contains a study of the purple membrane found in a species of halophilic bacterium, *Halobacterium halobium*, which shows it to have remarkable resemblance to retinal rods. The cells flourish only at extremely high concentrations of sodium chloride, and towards the end of the logarithmic growth phase there is a rapid accumulation of the purple membrane. Oesterhelt and Stoekenius have found that when the cells are caused to disintegrate by removal of salt, the fragments of the purple membrane are easily separated by centrifugation and washing from other parts of the membrane.

The absorption spectrum of the purple membrane is essentially that of a visual pigment, the maximum in the visible being nearer to that of the cone pigment, iodopsin, than rhodopsin. Denaturation with acid causes bleaching, the purple colour giving way to yellow. The bleached chromophore can be extracted with solvents, and is identifiable as retinal.

In SDS-gel electrophoresis, the dissociated membrane displays only one protein band, with an estimated molecular weight of 26,000. This is also the equivalent of protein found by nitrogen analysis to be present in the membrane for each equivalent of the chromophore. Thus the protein may be supposed to have a molecular weight of 26,000 (supported also by minimum numbers of the sparsely represented amino-acids, for example, one histidine residue corresponding 26,000 molecular weight), which is at the low end of the range of molecular weights at various times offered for rhodopsin. The analogy extends also to the attachment of the retinal to the protein: when reduced after bleaching with borohydride it can

no longer be extracted with solvents, and is evidently therefore attached in the native state by way of a Schiff base linkage to a lysine side chain. Oesterhelt and Stoekenius make the reasonable inference that the purple membrane may be a photoreceptor.

An X-ray diffraction study of the isolated purple membrane fragments by Blaurock and Stoekenius accompanies this characterization. The characteristic spacings for a membrane bilayer, corresponding to two dense layers 40 Å apart, are observed. Furthermore, there are reflexions that show the presence of a large hexagonal lattice, which must therefore be the form of surface packing of the protein molecules. Freeze-fractured specimens of whole cells show in the electron microscope evidence of a regular lattice on patches of the membrane (it being assumed that the cleavage plane is in the bilayer). Because these increase in number towards the end of logarithmic growth, it seems that they correspond to the purple areas. Light-diffraction patterns are also consistent with a hexagonal array.

From measurements of partial specific volume, Blaurock and Stoekenius have calculated that the unit cell in the purple membrane preparations contains some forty molecules of lipid and three or four molecules of protein. Because hexagonal packing in a plane demands that the unit cell contain three units, Blaurock and Stoekenius suggest that the protein is disposed in two layers rather than one. The visible absorption band of the chromophore shows strong dichroism, the long axis of the retinene lying in the plane of the membrane, exactly as found in retinal rods. This seems to be the first example of the existence of specialized functional areas in cell membranes.

Combustion Temperatures: The Future?

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An examination of the relative merits of burning at high and low temperatures leads to new proposals for designing future combustion systems for particular purposes so as to minimize pollution and optimize intensity and efficiency.

A 100 h.p. horse would be inconveniently large. In every other way, biological converters oxidize their fuel most efficiently, give rise to very little pollution and, indeed, dispose of those effluents which are caused by the low quality of their fuel in a convenient and elegant way. To go to the other extreme, a stabilized hydrogen-oxygen detonation could achieve the throughput equivalent to 100 h.p. in a disk of less than 1/4 cm diameter and of very small thickness. The main reason for this vast difference in volume, and hence in combustion intensity, lies in the steep dependence of the rate on temperature (see Fig. 1), the horse being some 3,000° C cooler than the detonation.

Even if we omit biological systems and confine ourselves to combustion reactions, the history of mankind has been intimately associated with this steep temperature dependence of the rate law. The observation that fuels and oxidants will not react at any appreciable speed when left alone at normal temperatures, but will do so at an alarming rate once they are raised to a high temperature, must have been one of mankind's first scientific discoveries. Since then the trend, very broadly, has been to ever higher temperatures and combustion intensities—that is conversion rates averaged over the volume of the combustion chamber—and under more controllable conditions. In view of the diminishing resources of fossil fuels (and their

potential usefulness for producing more interesting chemicals than carbon dioxide and water), of the falling costs of nuclear power generation and of the public awakening to the effects of pollution, it seems opportune to reassess the principles available for controlling the temperature in combustion reactions and to give serious consideration to any suggestion which may emerge for modifying practical systems.

The potential advantages of high combustion temperatures are: high reaction rates, high thermodynamic efficiency in the case of conversion to power, high heat transfer rates to surfaces on which dissociated species can recombine, higher ionization (which may be useful in particular applications such as magnetohydrodynamic control or power generation) and the opportunity of producing endothermic products by sudden quenching. These advantages are obviously over-riding for some purposes. For others, however, they are vitiated by the production of certain pollutants at high temperatures—particularly NO—and, chiefly, by the difficulty of handling such hot gases. These are so much above the melting points of the devices used to contain (for example, combustion chambers) and exploit (for example, turbine blades) them that one cannot realize the potential advantages in practice. In the case of heat transfer rates to surfaces there are applications—such as cutting, melting, welding, spheroidizing, and so on—in which the heat sink is not intended to remain intact. In most others, however, the melting of the confining walls would be disastrous. In this way, the chief advantages such as high combustion intensity or thermodynamic efficiency may be wasted if the high temperature cannot be maintained over the entire volume of the combustion chamber or converter. Yet the high temperature may nevertheless be necessary, in conventional combustors, in order to keep the combustion zone stable at the desired throughput. Thus we may be forced to accept the disadvantages of high temperatures—for example, excessive heat losses and formation of NO—yet at the same time have to cool the gases before they become acceptable, for example, to turbine blades.

The combustion temperature range is limited, at its lower end, by the least temperature at which a stable flame can be maintained—not much below 1,000° C under optimum conditions. This is determined by the interplay between the above mentioned temperature dependence of heat release rate with that of heat loss from the reaction zone. The upper limit is set, in practice, by the difficulty of containment within a solid container—or at best a liquid melt held against the cylindrical walls of its parent solid by rotation-induced centrifugal force. Ionization does not become adequate for magnetohydrodynamic confinement until enthalpies attain values which dwarf the contribution from the heat of combustion. There is also some much higher temperature limit beyond which the heat of reaction is not retrievable from the dissociated species until they are allowed to cool down. Both these limits depend on the application envisaged and do not apply in cases where maximum heat transfer—for example, for cutting or welding—is the desired objective.

In thinking about pre-mixed flames we tend to associate flame temperature absolutely with the initial reactant composition—that is, the fuel to air ratio. The adiabatic final flame temperature is given, for a given mass of mixture, by

$$Q_c = \int_{T_{unburnt}}^{T_{burnt}} C_p dT = [H_b - H_0]$$

where Q_c is the heat of combustion and C_p the heat capacity of the products at constant pressure. It is convenient to work in terms of enthalpy, H , so as not to have to take into account

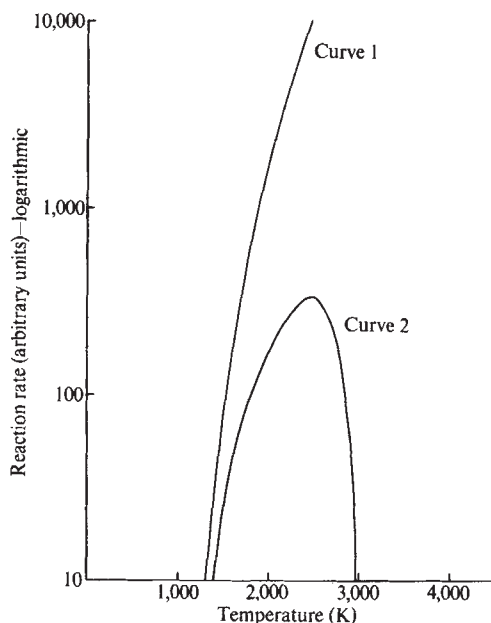


Fig. 1 Curve 1 shows the variation of reaction rate with temperature at constant pressure taken as $T^{3/2} \exp(-44 \times 10^3/RT)$. Curve 2 as curve 1 but with depletion and dilution of reactants, that is the temperature rise is due to reaction and/or mixing with hot products. It is curve 1 multiplied by $[1 - (T - 300)/(3,000 - 300)]^2$.

the effect of dissociation. For a simple flame (Fig. 2a), the reaction occurs over a range of temperatures close to T_b . If a converter is operated at this temperature its efficiency generally falls far short of the Carnot cycle value.

To maintain flexibility, however, it is better if we allow ourselves the variable of adding, subtracting, or recirculating some of the energy. Addition of energy to the reactants is perhaps the most obvious further degree of freedom and method of control. In its simplest form, we can preheat the reactants, though doing this by means of a solid preheater is limited to relatively low temperatures, both by the onset of preignition and by melting of the containing walls. Beyond this, we can use radiation, adiabatic compression, or electrical heating. As regards radiation, the difficulty is that in a gas (unlike in a dust cloud, for example) there is virtually nothing to absorb it. To succeed, the radiation flux has to be high enough to create a sufficient concentration of free electrons to absorb the energy. This can be achieved transiently at the focus of a Q-switched laser¹ but cannot, as yet, be done continuously. Adiabatic compression in continuous flow systems has been applied in the form of a stabilized detonation or, at least, shock initiated combustion^{2,3}. The burner here looks more like a supersonic wind tunnel, the gases emerging under-expanded from a nozzle and being rapidly heated across the standing shock wave. Experimentally, it is easier and cheaper to add the energy electrically by means of some kind of discharge and various devices based on a.c., d.c., magnetically rotated, arcs and radio frequency discharges have been discussed in the literature⁴.

In all these cases, if Q_a is the energy added, the final temperature, T_f , is obtained from

$$\int_{T_0}^{T_f} C_p dT = Q_c + Q_a = H_f - H_0$$

and the idealized thermodynamic efficiency corresponds to the higher temperature level. Again, this cannot be approached in practice, the additional problems here being associated with the containment and utilization of the much higher temperatures involved. Now there are some special purposes⁴ for which such augmented flames promise to be valuable, because of the large heat transfer from their dissociated products, their ability to stabilize very large reactant throughputs by generating specific radicals when they are used as pilot burners in high intensity combustion systems⁷, or for producing endothermic compounds by quenching their products. As the chemical energy from fuels tends as yet to be much cheaper than that from other supplies mentioned above, it is economically unacceptable for general purposes to heat these systems from an external source. The above mentioned increase in efficiency is small com-

pensation for having to supply Q_a as electrical power, particularly when losses in a compressor, or to arc electrodes, have to be taken into account.

Of greater economic viability is the concept of recirculating some of the combustion energy. Perhaps the most frequently used method of flame holding which operates on this principle is the recirculation of some of the hot combustion products. Widely used, particularly in propulsion devices, in its simplest form this requires only some baffle or change in contour of combustion chamber which will induce a vortex in the appropriate region. This kind of recirculation is, however, always accompanied by dilution of reactants with products and is useful only because of the particular form of the rate law which implies that the effect of increasing temperature is very considerably greater (see Fig. 1, curve 2) than the corresponding consumption of reactants and dilution with products, over a large part (at least the first 75%) of the conversion scale. The normal flame temperature (and hence the maximum efficiency of any converter) remains unaltered. Only the maximum throughput of the combustion chamber is increased because the volume giving a high combustion intensity is greatly enlarged by increased mixing over that in a laminar flame in which it is confined to a very narrow zone by the small scale of molecular transfer processes. Further improvements in mixing eventually lead to homogeneity and the idealization of the perfectly mixed reactor⁵ in which conditions are deemed to be uniform. The dilution effect, however, places an absolute maximum on the reaction rate attainable in this way. This is reached when the conditions of the apex of the reaction rate curve (2, Fig. 1) apply to the entire volume of a perfectly stirred chamber.

I shall now consider a simple alternative to which the above limitation does not apply—recirculating heat alone. In its simplest form, consider a burner whose premixed reactants are being preheated by a single encounter with the flame products (Fig. 2b). The enthalpy chart is included as a corrective to the impression that this device is suitable for producing infinite temperatures in the steady state. Indeed, once the steady state has set in, the exit temperature is the normal T_{burnt} value, calculated as above, since the outgoing enthalpy must be equal to the energy input. At this point it does not differ from the scheme based on recirculating hot products. In between the initial and the ultimate final stages, however, we have achieved a higher temperature on borrowed heat and this will produce a higher reaction rate (following a curve such as 1 in Fig. 1) or any of the other desirable features mentioned above. On taking the inlet tube round once more and postulating two perfect single stage heat exchangers (Fig. 2c) the maximum would correspond to double the borrowed enthalpy and so forth. To extend the concept, let us introduce a counter flow heat

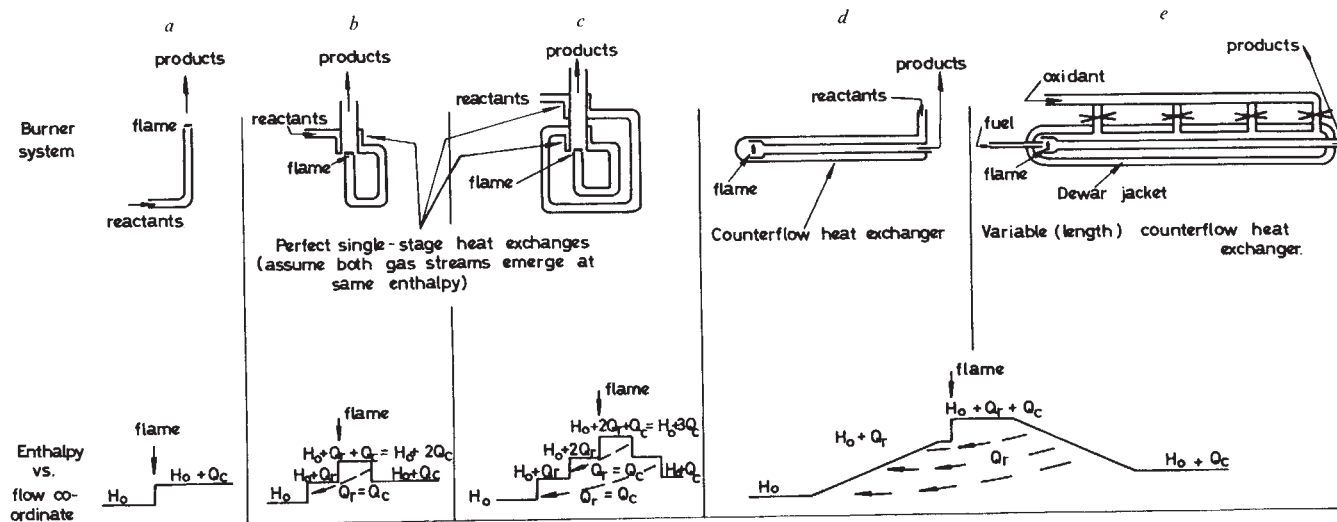


Fig. 2 Relation between enthalpy and flow coordinate for various burner systems.

exchanger (Fig. 2d) which we can make as long as we wish. In Fig. 2e some further refinements have been added: a vacuum flask surround to minimize heat losses from the device; a separate fuel and oxidant inlet, to prevent premature ignition; and a means of varying the heat transfer between outlet and inlet during operating conditions—all of which are perfectly feasible in practice. As regards the latter, there are, of course, more elegant methods of varying heat transfer than by the variation of effective length by selecting the appropriate inlet—the drawing is purely schematic. These devices dissociate the flame temperature from the initial composition. The enthalpy immediately beyond the reaction zone, H_c , is now given by

$$H_c - H_0 = Q_c + Q_r$$

being determined as much by the heat recirculated— Q_r —as by the input energy, Q_c . Moreover, the efficiency of a converter now operates on $(H_0 + Q_c + Q_r)$ instead of $(H_0 + Q_c)$. The output is therefore correspondingly higher and the overall efficiency can be increased by increasing Q_c , providing only that a small temperature differential is left to drive the heat transfer. In the case of a suitable converter it is possible to reject the thermodynamically unusable heat to the incoming gas—making the power cycle part of the heat exchanger. (For example, a thermoelectric generator could be operated between the combustion chamber and the inlet tube.) At first sight it seems surprising that such increases in overall efficiency should be possible without violating the laws of thermodynamics. In fact, we are merely “building a dam in the heat flow” and using the additional potential of releasing the chemical energy at its highest point.

Of course, in the case of mixtures of the now usual heats of reaction the heat exchangers in Fig. 2 would melt before more than 75% of the enthalpy could be recirculated. If, however, we can increase temperature arbitrarily by recirculating heat, we are no longer confined to such flames. At this point it will therefore be convenient to divide the entire combustion field into a high and a low temperature branch, the dividing line being drawn at the highest available melting point of any material of which a heat exchanger could be constructed. For the high temperature branch, if it is desired to raise the reaction temperature further, the principle of Fig. 2 must be interpreted schematically. Instead of the heat exchanger we might use a generator of electric power downstream which would supply an arc heater ahead of the flame, or we could make use of a detonation, in fact. In a steady state detonation, the degradation of momentum of the exit gases feeds the shock wave in front, which preheats the gas and is thus responsible for the high rate of conversion. Burners in which detonation is constrained to rotate in a torroid while the reactants are fed in, and products removed through slots in its side, have been proposed⁶.

For many applications, however, it may well be that the low temperature branch will appear more promising. Since the melting point of the container may not be exceeded even immediately downstream of the combustion chamber, this makes possible the use of extremely lean mixtures, or of very low grade fuels. In this way it would be possible, for example, to burn the ventilation air of gassy mines; it has been shown that a 1% mixture of methane and air will produce a self-sustaining flame, when it is preheated to less than 1,000° C, even though its own heat release contributes only another 250° C or so. This is, of course, an extreme case and, by varying the fraction of heat recirculated, the burner could be adapted to any mixture, so long as the normal adiabatic final flame temperature itself does not exceed the melting point. At the same time the scheme retains all its advantages of producing no high temperature pollutants, providing gases at temperatures acceptable to other mechanical components without an intermediate cooling stage and allowing some of the energy to be drawn at a higher temperature, and therefore efficiency, level, using a suitable thermoelectric converter or a Stirling motor, perhaps. An experimental quartz burner of this type (Fig. 3) instrumented with thermocouples to monitor gas temperatures in

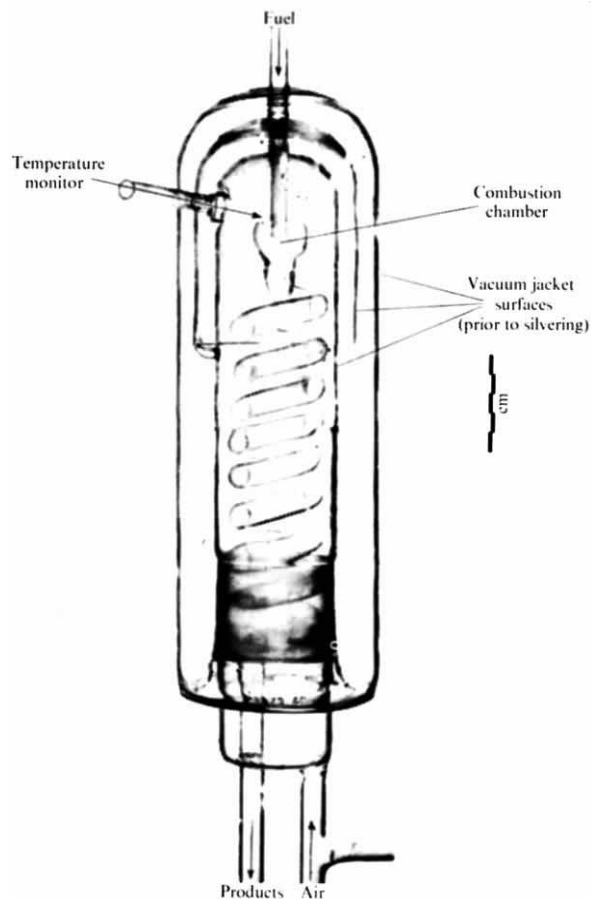


Fig. 3 Heat recirculating burner for lean fuel to air ratios.

various parts of the stream is now being operated in very lean mixtures of methane and air.

With the advent of coal dust and other fluidized bed reactors, the subject has been drifting in this direction during the past few years. A fluidized bed does, of course, act as a very efficient heat exchanger. In the more remote future it may well be, when further improvements in the combustion intensity of low grade fuel mixtures are required, that they will be achieved by hybridizing the high and low temperature branches. It has become apparent already that an augmented flame pilot producing radicals at high temperatures would greatly help to stabilize a large throughput of mixtures of low heats of combustion⁷, particularly if these have already been preheated by the recirculation scheme.

Thus, once we can control the reaction temperature independently of the heat of combustion of the mixture by one of the methods mentioned, it becomes feasible and desirable to design combustion chambers for specific purposes. In this way we can increase combustion intensity and converter efficiency, minimize pollution, burn low grade fuels efficiently and generate product gases at a temperature acceptable to machinery downstream without either subsequent cooling or having to reduce reaction temperature and hence throughput.

I thank Drs A. R. Jones, D. R. Hardesty and J. B. Cox for helpful discussions.

- Schwar, M. J. R., and Weinberg, F. J., *Combustion and Flame*, **13**, 335 (1969).
- Gross, R. A., *Amer. Rocket Soc. J.*, **29**, 63 (1959).
- Nicholls, J. A., and Dabora, E. K., in *Eighth Intern. Symp. Combustion*, 644 (Williams, and Wilkins, Baltimore, 1962).
- Lawton, J., and Weinberg, F. J., *Electrical Aspects of Combustion* (Oxford University Press, 1969).
- Longwell, J. P., and Weiss, M. A., *Ind. Eng. Chem. (Ind. Intern. ed.)*, **47**, 1634 (1955).
- Voitsekhovskii, B. V., *Sov. Fiz. Dokl.*, **4** (trans. from *Dokl. Akad. Nauk SSSR*, **129**, 1254 (1959)).
- Harrison, A. J., and Weinberg, F. J., *Proc. Roy. Soc., A*, **321**, 95 (1971).

Obsidian Source Identification by Fission Track Analysis

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Fission track analysis has been used to determine the age and uranium content of obsidians from sources in south-east Europe and Anatolia, and from archaeological deposits in mesolithic levels at the Franchthi Cave in southern Greece. It is confirmed that the Franchthi obsidian came from the Aegean island of Melos. This is the earliest positive indication available for maritime travel, and carries the history of seafaring back a thousand years.

Obsidian, or volcanic glass, was widely traded in prehistoric times, and the investigation of the trading pattern has long been a preoccupation of archaeologists and prehistorians. To characterize the obsidian, of which an artefact is made, physical or chemical properties of the material must be recognized which distinguish it as deriving from a specific natural source. Optical spectroscopy, X-ray fluorescence, and neutron activation are among the analytical techniques which have been used¹ for this purpose.

Trace element analysis usually works well: when concentrations of appropriate elements (for example, barium against zirconium) are plotted, obsidians from single sources form distinct clusters, well separated from one another. Difficulties, however, arise when obsidians from several different sources show convergent compositions. This has been observed² in Greece, where obsidians from the sources on the Cycladic island of Melos in the central Aegean Sea resemble in composition those from the Dodecanesian island of Giali in the eastern Aegean, lying 240 km east of Melos. As a further complication, both resemble, in their trace element composition, obsidian from the Tokaj region of Hungary, 1,300 km to the north-west, and from the central Anatolian source at Acigöl near Kayseri, 1,000 km to the east of Melos. These are the sources nearest to the Aegean, and it has been sug-

gested that Hungarian or Anatolian obsidian reached Greece by trade in prehistoric times.

When obsidian artefacts are found in very early archaeological contexts in mainland Greece, an Aegean source for the material (in Melos or Giali) seems the most likely. But clearly the possibility of trade with Anatolia or with Hungary cannot be ruled out, especially because recent work³ has confirmed that there was already an extensive early trade in seashells between the Aegean and the Balkans, in neolithic times. The possibility also remains, although it is remote, that obsidian was also obtained from some Aegean source at present unknown.

To overcome these problems it seemed useful to consider other variables which might discriminate between sources. The date of eruption in which the obsidian was formed is one such parameter, and the fission-track analysis method of age determination was a possible approach. The method, of course, has its limitations: two geographically remote eruptions may have occurred at roughly the same time; or, conversely, successive eruptions may have taken place in a given area over a long time. This is known to be the case in the Lipari islands off the coast of Sicily in the Tyrrhenian Sea, where dated archaeological deposits lie intercalated between obsidian-bearing strata. The same may hold for the Acigöl source, where several obsidian flows are known in the source region. In such cases the determination of a further parameter, the uranium content of obsidian samples, which is an integral part of the fission track analysis technique, may be useful in supplementing other criteria available for source identification. It may be added here that our purpose has not been to date the use of the artefacts (which are made by chipping or flaking, and not melting, of the obsidian). That only becomes possible if the artefact has been heated to high temperatures during its use⁴, thus annealing out the then-existing fission tracks and, effectively, re-setting the clock.

Our attention has been focused on obsidians designated as Group 1 on the basis of the existing trace-element classification of Cann and C. R.⁵, the aim being to distinguish between the various members of this group (which comprises obsidians from Melos and Giali in the Aegean Sea, as well as those from central Anatolia and the Carpathian Range in Hungary). Samples from these sources, the trace-element composition of which had previously been determined by

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optical spectrography^{2,6}, were selected for analysis (Table 1). Furthermore, three samples were chosen from the "mesolithic" (that is, pre-farming) levels at the Franchthi Cave in South-East Greece, excavated by Professor T. W. Jacobsen⁷. Radiocarbon dates for this period at Franchthi, lying in the eighth and seventh millennia BC, establish this as the earliest prehistoric context in the Aegean where obsidian has been found. For instance, stratum H-1, A117, from which obsidian was examined, yielded dates of 7527 BC and 6792 BC on the 5568 year ¹⁴C half-life.

Table 1 Ages and Uranium Contents of the Obsidians Investigated

Obsidian sample *	Age (m.y.) †	U-content (p.p.m. by weight) ‡	
		Primary detection method	Auxiliary detection method
Hungarian			
Borsod (60)	3.86 ± 0.24	8.6	7.7
Borsod (22)	3.37 ± 0.27	8.9	
Central Anatolian			
Bor (100)	2.29 ± 0.32	2.5	
Acigöl (269)	1.95 ± 0.33	3.5	
Acigöl (156)	8.14 ± 0.59	9.0	
Aegean			
Giali (390)	2.01 ± 0.26	11.1	
Giali (218)	8.04 ± 0.65	4.1	
Melos, Adhamas (120)	8.95 ± 0.94	4.7	
Melos, Adhamas (25)	8.54 ± 0.73	4.8	
Melos, Dhemenegaki (119)	8.35 ± 0.72	4.9	
Melos, Dhemenegaki (116)	2.36 ± 0.53§	—	9.1
Franchthi cave (S. Greece)			
Sample 6936 (H-1, A117)	8.48 ± 0.55	4.6	
Sample 7077 (H-1, A123)	8.82 ± 0.57	4.1	
Sample 7066 (H-1, A99)	9.33 ± 0.60	4.4	

* The code numbers are in accordance with those in refs. 2 and 6.

† All ages are determined by using equation (1) (see text), based on the primary detection method, except for sample 116. No corrections are necessary for thermal lowering of ages (see Table 2). The quoted uncertainties in ages are made up of statistical (Poisson) errors, background subtraction errors, and fluence uncertainties (± 5%).

‡ The U-content is determined in all cases by the primary detection method (that is by counting natural and induced tracks in the sample itself), except in the case of sample 116 where the auxiliary detection method was used (placing an external detector to receive induced fission fragments emanating from the sample). In the case of sample 60, both methods were used for comparison purposes. The uncertainty in uranium content values is estimated to be ~10%.

§ The age is based on natural track counting in conjunction with uranium-content determination by the auxiliary detection method in view of the poor optics of the sample. These two factors result in the unusually high error quoted (± 22.5%). It is hoped to make a more precise determination of this age later.

Fission Track Analysis

The fission track analysis method has been used by several authors, both to determine the ages of obsidians (see, for example, refs. 8–11) and to obtain their uranium (and thorium) content^{9,11–12}. It can be readily shown (refs. 13 and 14, and unpublished work of H. A. K. and S. A. D.), that the age A (in years), and the uranium content C_u (in p.p.m. by weight) in a material, are given by

$$A = (\rho_n/\rho_i) \cdot 5.01 \times 10^{-8} \cdot F \quad (1)$$

$$\text{and } C_u \text{ (p.p.m.)} = 9.445 \times 10^7 \times \rho_i / (D \cdot R_{ap} \cdot \cos^2 \theta_c \cdot g \cdot f(t) \cdot F) \quad (2)$$

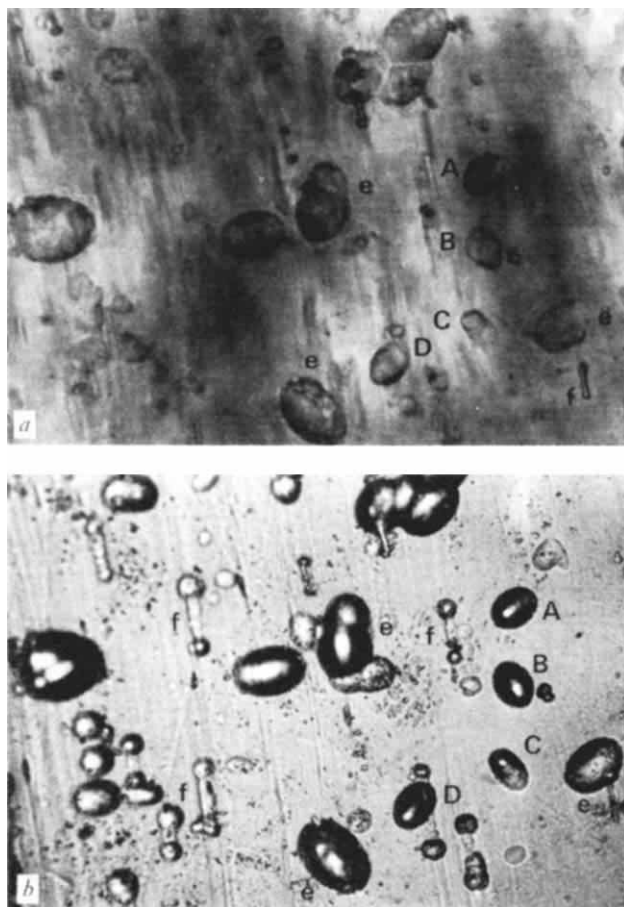


Fig. 1 Contrast between the two modes of microscopic viewing. The same field of view in the Dhemenegaki sample 119 is shown in the two photomicrographs under (a) transmitted light mode and (b) "epi-illumination" (reflected-light) mode. In a, the poor optics of the sample (notice streaks of varying colour and refractive index) results in ill-defined fission etch pits, which are difficult to distinguish from spurious pits and defects. In b, epi-illumination brings out the genuine fission etch pits (A, B, C, D) quite clearly. Pits marked e are spurious etch pits on the adopted criteria; configurations marked f are believed to be feldspar crystals the extremities of which appear to offer preferential etching channels to the acid as seen in b; in a the feldspar crystals are in deeper layers which have not been etched).

Here ρ_n and ρ_i are the densities of etch pits per cm^2 produced by spontaneous and induced fission, respectively, the latter being induced by a fluence F of thermal neutrons per cm^2 , D is the density (g cm^{-3}); R_{ap} , the "apparent range" of an average fission fragment; θ_c the "critical angle" of etching; g a geometrical factor; and $f(t)$ the "prolonged etching factor" for the material. We take the value of $D \times R_{ap}$ to be the same as found¹⁴ for a "reference glass" (U-2) containing a known amount of uranium (namely, $2.41 \text{ g cm}^{-3} \times 15.2 \mu\text{m}$), and $g=1$ (that is "4 π geometry": the contribution of fission fragments from both sides of each etched surface being taken into account). The numerical factors in equations (1) and (2) result from various natural constants; in particular the fission decay constant of ²³⁸U has been taken to be $8.42 \times 10^{-17} \text{ yr}^{-1}$.

The critical angle, θ_c , is defined as the minimum inclination, with the surface, of a track that can be successfully etched. We have determined the value of θ_c for (Hungarian) obsidian (unpublished work of H. A. K. and S. A. D.), using an external collimated fission source of ²⁵²Cf, to be $26^\circ 00' \pm 30'$.

The prolonged etching factor $f(t)$, first introduced by H. A. K. and S. A. D. (unpublished work; see also ref. 14) is defined as the ratio ρ/ρ_0 , where ρ is the track density observed on etching the sample for time t , and ρ_0 is the extrapolated value

corresponding to zero etching time. The factor cancels out in equation (1) if identical etching conditions are used to determine both ρ and ρ_i , but not in equation (2). The value of $f(t)$ for obsidian is found to be 1.17 at $t=60$ s (on etching in ≈ 40 vol. %HF at 13°C).

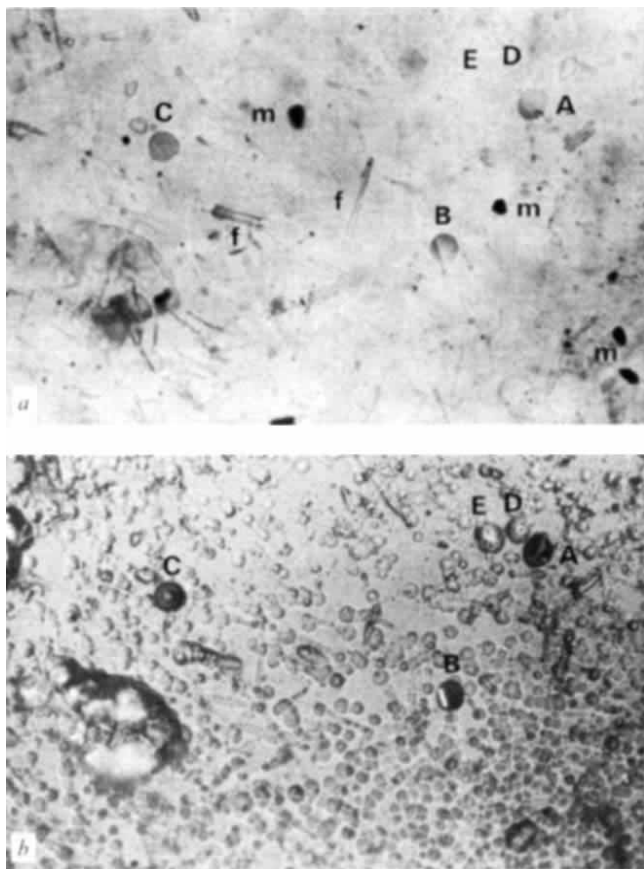


Fig. 2 Acigöl sample 269 observed under (a) transmission mode and (b) epi-illumination mode of microscopic viewing. In *b* the genuine fission etch pits (A, B, C, D, E) on the top surface are easy to pick out, whereas they remain indistinct in *a* which covers the same field of view. The needle-like objects marked *f* are feldspar crystals; the black inclusions marked *m* are believed to be magnetite or other iron ore crystals in the body of the sample (seen in *a*). Such crystals and inclusions, when they cross the top surface, get etched and result in the spurious pits seen in *b*. That most of these pits and other defects are unconnected with fission damage is confirmed by their survival on high temperature annealing (see text). The bright line in etch pit B seen in *b* is the reflexion from a feldspar crystal lying in a different plane.

Although the fission track analysis technique is simple in principle, considerable difficulties were encountered in applying it to obsidians. Several samples were optically very "dirty", containing dark streaks of varying colours and inclusions of crystals of magnetite, feldspar and the like. The latter were found to lie along "flow bands", indicating that feldspar crystals had formed while the obsidian was still partially fluid and in the process of cooling down. On etching in hydrofluoric acid, when a feldspar crystal (or less frequently, a limb of a skeletal magnetite crystal) crossed the etched surface, it seemed to offer a preferential etching channel to the acid, thus yielding an etch pit which was often difficult to distinguish from a genuine fission-fragment etch pit. That most of the pits and other defects were unconnected with fission damage was confirmed by the observation that, in spite of heating

the obsidian samples for 1 h at 700°C , the number of etch pits subsequently produced remained largely unchanged. Fleischer and Price have shown¹⁰ that even pure fused silica loses all fission tracks under these annealing conditions (tektite glass requires^{10,15} only $\sim 500^\circ\text{C}$ for 1 h for this to happen).

These problems of extraneous, but optically similar, etch pits and enclosed "blisters" and other defects were largely overcome by adopting the "epi-illumination" mode of viewing, instead of the transmitted-light mode used hitherto. The microscope used was a Zeiss 'Photomicroscope'. Figs. 1 and 2 show two pairs of typical photomicrographs under these two viewing conditions.

Ages and Uranium Contents

Table 1 records the ages and uranium contents of obsidian samples from Hungary (Borsod region), central Anatolia, and the Aegean Sea (three different locations). It is interesting to note that although the two samples from Hungary have, within experimental errors, identical ages of formation, the Aegean and the central Anatolian samples show a certain sequential eruption. Thus the two samples (120 and 25) from Adhamas as well as one each from Acigöl (156), Giali (218) and Dhemenegaki (119) all show eruption ages between 8 and 9 m.y., thus suggesting a general volcanic activity in the region at that time. A further simultaneous activity in the whole area (indicated by the central Anatolian samples 100 from Bor and 269 from Acigöl; sample 390 from the island of Giali; and sample 116 from Dhemenegaki on the island of Melos) seems to have taken place about 2 m.y. ago. These conclusions are of considerable potential interest to geologists and petrologists. From our point of view, however, the coincidence of eruption dates (~ 2 m.y. and ~ 8 m.y.) in the various locations of interest is a little unfortunate, because it makes clear-cut identification of obsidian sources more difficult. This coincidence is, nevertheless, fortuitous and the principle of the method remains unaffected.

A further variable at our disposal is the uranium content. Here, the two Hungarian samples are found (within experimental errors) to have identical uranium contents as well as eruption ages; the same applies to three of the samples from the island of Melos (120 and 25 from Adhamas, and 119 from Dhemenegaki). The two obsidian samples each from Acigöl, Giali and Dhemenegaki can, however, be clearly differentiated on the basis of both age and uranium content. The uranium content of sample 116 from Dhemenegaki (which was optically very dirty) has been determined by H. A. K. and S. A. D. (unpublished work) using the auxiliary detection method (by irradiating the sample, while in contact with an external plastic detector, in a nuclear reactor). The U-content (9.1 p.p.m.) is quite distinct from the other Dhemenegaki sample (119), as is the age.

In comparing ages, it is necessary to ensure that some of them have not been thermally lowered. It is known (see, for example, refs. 15 and 16) that the application of heat results in track fading, leading to an apparent reduction of age. This can be corrected¹⁴⁻¹⁶ by examining the etch pit diameters (in identical etching conditions) of freshly induced fission and spontaneous (or natural) fission: for fading of tracks is accompanied by a general diminution of the resulting etch-pit diameters. Table 2 shows the results for induced and spontaneous fission in the obsidians studied by us. Application of t test to each of the distributions listed shows that no significant diminution of etch-pit diameters has taken place. The ages recorded in Table 1 can thus be taken to be the true ages.

Franchthi Cave Obsidian

It is clear from the foregoing that, in favourable circumstances, the eruption age and uranium content of obsidian samples can serve as source identification criteria. As a test

case, a heuristic assay was made on three obsidian samples from mesolithic levels of the Franchthi Cave in the Argolid, southern Greece. Nearly all the obsidian artefacts and waste flakes at Franchthi show the pearly lustre common in obsidian from both the Melian sources at Adhamas and Dhemenegaki. Melos, lying 120 km south-east of Franchthi, is the nearest known source of workable obsidian. Preliminary spectrographic analyses by Dr J. E. Dixon of the University of Edinburgh place the Franchthi specimens in Group 1 of the existing classification, which is consistent with a Melian origin. The very early date of the Franchthi material, and the lack of other evidence for maritime travel in the eighth or seventh millennium BC made a fresh approach, by fission track analysis, desirable.

Table 2 Comparison of Etch-pit Diameters of Spontaneous and Induced Fission Tracks in Obsidian Samples in Order to Judge whether Thermal Lowering of Ages has Taken Place

Obsidian sample *	Etching time (s)	Mean etch pit diameter (μm) (and the number of pits measured) ‡	
		Spontaneous fission	Induced fission
Hungarian			
Borsod (60)	90	14.16 ± 2.30 (80)	14.39 ± 2.22 (67)
Borsod (22)	60	9.74 ± 1.32 (80)	10.01 ± 1.55 (70)
Central Anatolian			
Acigöl (269)	60	8.81 ± 1.49 (100)	9.06 ± 1.57 (100)
Acigöl (156)	60	8.93 ± 1.46 (120)	9.06 ± 1.37 (120)
Aegean			
Giali (390)	60	8.96 ± 1.23 (103)	8.90 ± 1.34 (112)
Giali (218)	60	8.76 ± 1.16 (89)	8.71 ± 1.05 (81)
Melos, Adhamas (120)	90	11.51 ± 1.28 (103)	11.29 ± 1.29 (98)
Melos, Adhamas (25)	60	8.63 ± 1.10 (90)	8.61 ± 1.12 (88)
Franchthi cave (S. Greece)			
Sample 6936 (H-1, A117)	60	8.56 ± 1.22 (95)	8.70 ± 1.19 (98)
Sample 7077 (H-1, A123)	60	8.86 ± 1.32 (105)	8.85 ± 1.25 (105)
Sample 7066 (H-1, A99)	60	9.00 ± 1.14 (91)	8.98 ± 1.23 (91)

No significant difference in diameters is found in any sample, as verified by the application of t test to the means and the standard deviations of the distributions.

* The code numbers are in accordance with those in refs. 2 and 6.

† Etching is always carried out in ≈ 40 vol. % HF at 13°C .

‡ The numbers of etch pits measured are shown in parentheses immediately after the values of diameters. For elliptical pits, major axis has been measured instead of diameter. The errors shown are standard deviations of "single observations".

Table 1 records the results for age and uranium content from these samples. It will be seen that not only do the ages and U-contents agree between the three samples, but they are also very similar to those for Melian obsidian from Adhamas (samples 120 and 25), and for one of the Dhemenegaki samples (119). They are also compatible with one of Giali samples (218) but not with any of the central Anatolian or Hungarian samples.

The results are thus entirely consistent with an origin in the Melian sources at Adhamas or Dhemenegaki. They exclude a Hungarian or central Anatolian origin, and are particularly useful in reducing the likelihood of an origin in some source

in mainland Greece so far unknown, a possibility suggested by some scholars¹⁷. Although the possibility of unrecorded obsidian sources in mainland Greece could not be excluded, it already seemed unlikely that such a source would fortuitously produce material resembling that of Melos both in appearance and trace element composition. That it should further match the Melian material both in age and uranium content (which the prehistoric finds from Franchthi now do) would be an altogether remarkable coincidence. We therefore suggest that the possibility of such an origin for the Franchthi material, from an unknown source, may now be excluded.

The conclusion that the Franchthi obsidian came from Melos is of great interest. Hitherto, the earliest evidence for a traffic in Melian obsidian was from early neolithic sites at Knossos in Crete, at Sesklo in Thessaly and at Nea Nikomedeia in Macedonia, all a full millennium later. It seemed that in the Aegean, as in the Near East, the obsidian trade began in earnest at the time of the development of farming. Now it seems that the hunters and fishers of southern Greece were already, by the beginning of the seventh millennium BC, and perhaps earlier, reaching Melos by boat. The occurrence of obsidian in successive layers at Franchthi suggests, moreover, that Melos was reached on several occasions. As Jacobsen points out⁷, it may be relevant that obsidian first occurs at Franchthi at about the same time that its inhabitants were beginning the intensive exploitation of marine fish. Seafaring and trade may have been the consequence of the intensive use of the sea as a food source.

The Franchthi Cave obsidian is the earliest positive evidence available, from anywhere in the world, of the transport of goods by sea. The very rare obsidian from early neolithic Cyprus, brought by sea from the central Anatolian sources¹⁸, is not found there until 1,500 yr later. The corroboration by fission track analysis of a Melian origin for the Franchthi Cave obsidian thus carries back the history of seafaring by more than a thousand years.

We thank Professor T. W. Jacobsen for allowing C. R. to study the Franchthi Cave obsidian, and for arranging (with the cooperation of the Greek Archaeological Service) that selected samples be made available for analysis; Mr E. D. Lacy for advice on the chemistry of obsidians; and the SRC and the staff of HERALD reactor at AWRE, Aldermaston, for irradiation facilities. H. A. K. and M. T. thank the Pakistan AEC and Colombo Plan authorities, and the Government of Pakistan, for financial support.

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- Cann, J. R., Dixon, J. E., and Renfrew, C., in *Science in Archaeology* (edit. by Brothwell, D., and Higgs, E. S.), 578 (Thames and Hudson, second ed., 1969).
- Renfrew, C., Cann, J. R., and Dixon, J. E., *Ann. Brit. School Archaeol. Athens*, **60**, 225 (1965).
- Shackleton, N., and Renfrew, C., *Nature*, **228**, 1062 (1970).
- Fleischer, R. L., Price, P. B., Walker, R. M., and Leakey, L. S. B., *Nature*, **205**, 1138 (1965).
- Cann, J. R., and Renfrew, C., *Proc. Prehist. Soc.*, **30**, 111 (1964).
- Renfrew, C., Dixon, J. E., and Cann, J. R., *Proc. Prehist. Soc.*, **32**, 30 (1966).
- Jacobsen, T. W., *Hesperia*, **38**, 343 (1969).
- Watanabe, N., and Suzuki, M., *Nature*, **222**, 1057 (1969).
- Kaneoka, I., and Suzuki, M., *J. Geol. Soc. Japan*, **76**, 309 (1970).
- Fleischer, R. L., and Price, P. B., *Geochim. Cosmochim. Acta*, **28**, 755 (1964).
- Suzuki, M., *J. Anthropol. Soc. Nippon*, **78** (1), 50 (1970).
- Hair, M. W., Kaufhold, J., Maurette, M., and Walker, R. M., *Radiat. Effects*, **7**, 285 (1971).
- Durrani, S. A., and Hancock, D. A., *Earth Planet. Sci. Lett.*, **8**, 157 (1970).
- Durrani, S. A., and Khan, H. A., *Nature*, **232**, 320 (1971); *Nature Physical Science*, **232**, 175 (1971).
- Durrani, S. A., and Khan, H. A., *Earth Planet. Sci. Lett.*, **9**, 431 (1970).
- Storzer, D., and Wagner, G. A., *Earth Planet. Sci. Lett.*, **5**, 463 (1969).
- Theochares, D. R., *I Avgi tis Thessalikis Proistorias*, 79 (Philarchaios Etairia, Volos, 1967).
- Renfrew, C., Dixon, J. E., and Cann, J. R., *Proc. Prehist. Soc.*, **34**, 325 (1968).

Evolution of the Scotia Arc

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In this article the evolution of the Andean–West Antarctic Cordillera, and its subsequent bending, disruption, and fragmentation in the Cenozoic to form the Scotia Arc, is documented by geological evidence.

THE suggestion that the Andean mountain chain reappears in the Antarctic Peninsula¹ (Fig. 1) has in the past been supported by geological similarities^{2,3} and the discovery of the discontinuous arcuate submarine Scotia Ridge (Figs. 1 and 2) that joins the two continents⁴. Significant detail has been added in the past thirty years to strengthen this hypothesis.

In earlier models for the evolution of the Scotia Arc, as the whole feature is generally known, the islands of the North and South Scotia Ridges (Fig. 2) were assumed to be parts of an originally straight north–south Cordillera displaced eastward relative to South America and Antarctica during the drift of these continents westward relative to Africa^{5–9}. Recently, however, it has been suggested that the present east–west trend of the North and South Scotia Ridges is primary^{10–12}. Our review¹³ of the published data, and of unpublished data resulting from recent field work^{14–17} (also personal communications from G. Grikurov, 1970; M. K. Rickard and K. T. Burns, 1970; J. Stewart, 1971; and E. González P., 1971), leads us to support the concept of an essentially rectilinear Andean–West Antarctic Cordillera. In what follows, we shall reconstruct this Cordillera in some detail. The bending, disruption and fragmentation which formed the Scotia Arc imply less shortening of the Pacific margin than has been assumed¹⁸.

Geological Considerations

The Scotia Ridge system is discontinuous, and locally emergent, forming small, high and extremely rugged islands (Fig. 2). The islands on the east–west trending North and South Scotia Ridges consist mainly of pre-Cenozoic rocks of continental character that can be correlated with those in both southern South America and the Antarctic Peninsula^{2,3,5,6,9,13,16,22–24}. The islands of the South Sandwich Ridge, which closes the arc in the east, are wholly volcanic (most are active volcanoes)²⁵, and lie in a seismically active belt to the west of the South Sandwich Trench (Fig. 1).

The basin of the western Scotia Sea and Drake Passage (47° W to 70° W) are underlain by “normal” oceanic crust^{26,27}, but in some eastern parts of the basin there is a layer with P-wave velocity 7.5 km s⁻¹ at the base of the crust^{26–28}. Refraction profiles across the Burdwood Bank and the platform south of the South Orkney Islands (Fig. 2) reveal layers that are interpreted to be continental^{26–30}. South Georgia

and the South Shetland Islands (Fig. 2) are underlain by approximately 4 km of low velocity rocks (possibly consolidated sediments) over high velocity crustal material that may be either continental or oceanic^{26,31}.

The structural framework of the Scotia Arc in terms of plate tectonics is not entirely clear. It is generally assumed that the northern boundary of the Antarctic crustal plate lies along the Chile Rise and the South Sandwich Fracture Zone, both apparently consisting of fracture zones connecting short spreading-ridge segments^{32,33}. But neither the North and South Scotia Ridges, nor the continental margin of southernmost South America have marked seismicity now (Fig. 1). Consequently the intense seismicity associated with the South Sandwich Trench seems to be anomalous. The problem remains of why there is relative motion between the American and Antarctic plates only along the South Sandwich Fracture Zone and the South Sandwich Arc (Fig. 1). Recently magnetic evidence has been presented for a late Cenozoic–Recent spreading-ridge, trending north–east–south–west in the Drake Passage, and for another situated immediately west of the South Sandwich Islands (Fig. 1) that trends north–south. The presence of the latter, if still active as proposed, helps to account for the seismicity of the South Sandwich Trench^{10,12,21}. Reconstructions based on the former have led to an hypothesis that the Andean–West Antarctic Cordillera formed an easterly directed cusp in the area of the Drake Passage prior to 20–25 m.y. ago^{10,12}.

The geology of the Scotia Arc region can be simplified by emphasizing what seem to us to be the fundamental tectono-stratigraphic units. This admittedly involves certain assumptions in correlation¹³. The units are

Cenozoic sedimentary and volcanic deposits

Late Mesozoic–Cenozoic plutons (the “Andean” igneous complex)

Jurassic and Cretaceous sedimentary and volcanic deposits

Late Palaeozoic sedimentary rocks

Metamorphic basement complexes (? Palaeozoic; possibly Pre-Cambrian)

The metamorphic basement complexes (mostly low to medium grade parashists) and the late Palaeozoic sedimentary rocks (mostly graywackes and shales) were both deformed prior to the deposition of the unconformably overlying mid to late Jurassic rocks; the sediments were deformed during the early Mesozoic Gondwanian Orogeny (known elsewhere in Antarctica as the Ellsworth³⁴ or Weddell³⁵ Orogeny), and the metamorphics probably earlier^{13,22,36,37}. The age relations of the metamorphic basement and the Palaeozoic sediments are not certain, but recent Rb–Sr analysis of cores from the metamorphic basement east of the Andes^{38,39} support a pre-late Palaeozoic age for the metamorphism. The Jurassic and Cretaceous rocks, including a thick sequence of mid to late Jurassic calc–alkaline volcanics, were deformed and metamorphosed with variable intensity^{13–16} in the late Mesozoic–

earliest Cenozoic Andean Orogeny. They were intruded by pre-, syn-, and post-tectonic plutons of the batholithic "Andean" igneous complex^{13,15,16}.

Cenozoic sedimentation was scattered and locally interrupted by volcanism. No ages older than 4 m.y. (ref. 25) have been recorded from the South Sandwich Islands. But correlation of the magnetic anomalies immediately west of the islands^{10,21} implies that volcanism began at least 8 m.y. ago.

The tectono-stratigraphic units can be traced in South America in continuous belts trending parallel to the Andean Cordillera. They are irregularly distributed in the islands of the Scotia Ridge and the Antarctic Peninsula (Fig. 2). The two main physiographic and tectonic provinces of southern South America are the Andean Cordillera and the extra-Andean lowlands to the east. The lowlands are underlain by the virtually undeformed Mesozoic and Cenozoic sediments and volcanics of the Magallanes Basin (Fig. 2) resting on a Palaeozoic or older metamorphic basement^{38,39}. The Cordillera consists of deformed late Jurassic and Cretaceous volcanics and

sediments that unconformably overlie the metamorphic basement complex, and are cut by plutons of the batholithic complex. In Tierra del Fuego the Jurassic volcanic deposits are disposed in an anticline with the metamorphic rocks in the core (M. K. Rickard and K. T. Burns, personal communication, 1970). This anticline probably continues northward along the Cordillera (unpublished data of Empresa Nacional del Petroleo, Instituto de Investigaciones Geologicas, and I. W. D. D.), although the structure is complicated by thrusting from the Pacific side⁴⁰ (also unpublished data of Empresa Nacional del Petroleo, M. K. Rickard and K. T. Burns, and I. W. D. D.). Cretaceous flysch-type sediments flank the High Cordillera, those on the Pacific side being interbedded with mafic volcanics. The "Andean" igneous complex forms much of the Pacific side of the Cordillera. Late Palaeozoic sediments of the Madre de Dios Basin occur on the west coast at 50°–52° S (Fig. 2).

Our detailed reconstruction of the Andean–West Antarctic Cordillera prior to the inception of the Scotia Arc is based largely on the recognition of the tectonic provinces of southern

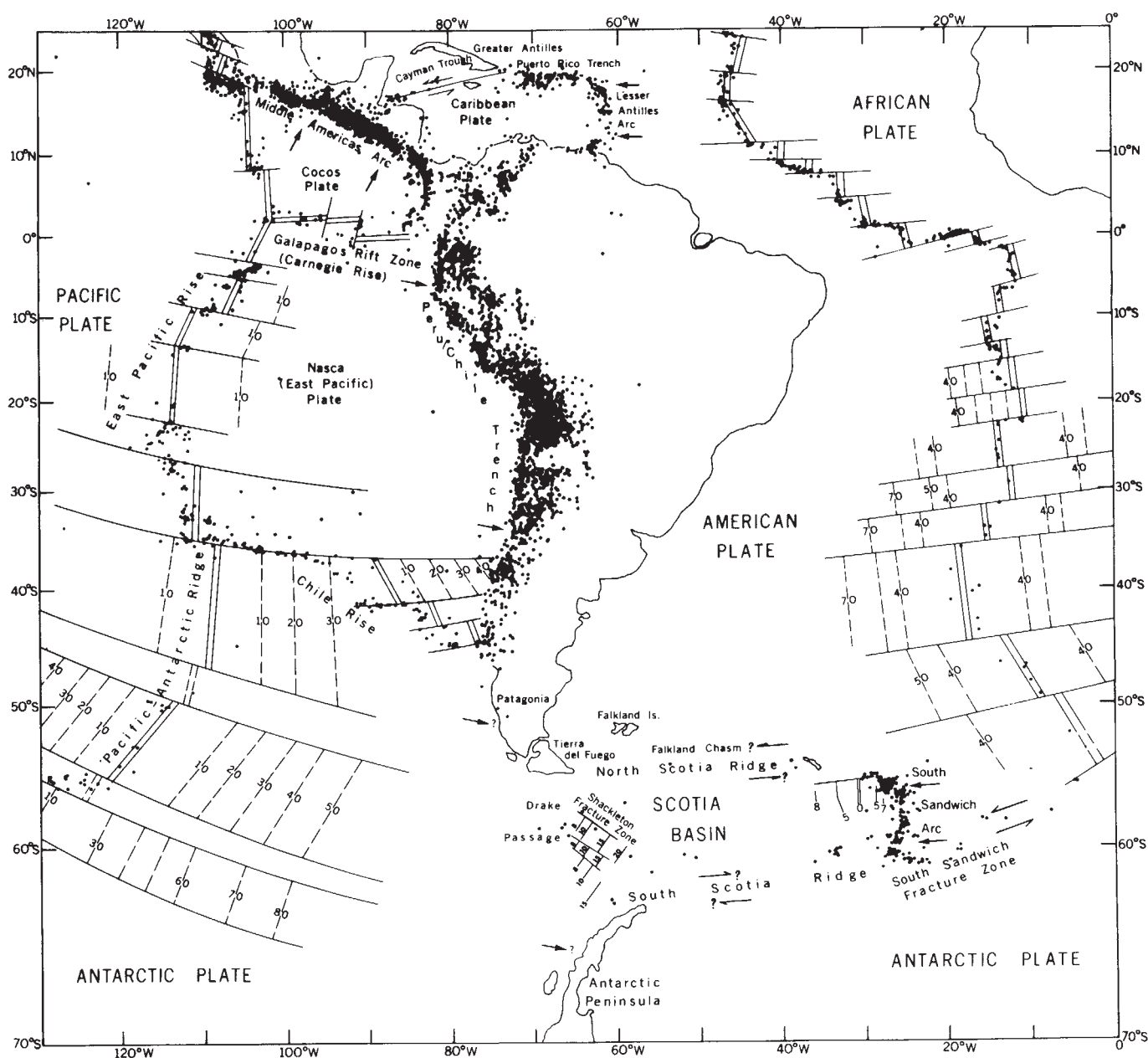


Fig. 1 The geotectonic setting of the Scotia Arc. Epicentres of earthquakes at depths of 0–700 km for the period 1961–1967 (ref. 19) are shown by black dots. The approximate age in millions of years of the floor of the Atlantic and Pacific Oceans, and of the Scotia Sea, as inferred from marine magnetic anomalies^{10,20,21} is also shown. Arrows indicate inferred relative motion of the lithospheric plates on which they are drawn.

South America along the North and South Scotia Ridges, and in the Antarctic Peninsula. Extrapolation of the onshore geology (Fig. 2), our interpretation of the seismic refraction profiles^{26,29}, and dredging⁴¹, indicate that the shallow Burdwood Bank immediately east of Tierra del Fuego is a structural continuation of the extra-Cordilleran Mesozoic-Cenozoic sedimentary basin. Refraction data^{26,29} also show higher velocities in the basement of the basin that underlies the bank and forms its southern margin. This (seismic) basement is likely to be the Jurassic volcanics that underlie the sedimentary succession of the extra-Cordilleran basin in Tierra del Fuego^{29,42}, and which are known along strike as far east as Isla de los Estados (Fig. 2). Lack of appreciable magnetic expression along the southern margin of the bank^{12,27} is not surprising, for the volcanics are largely felsic^{13,15,16,40,42}.

The metamorphic rocks of islets called the Black and Shag Rocks, the flysch-type rocks of early Cretaceous age intruded by an "Andean"-type igneous complex on South Georgia and the "Andean"-type igneous rocks of the Clerke Rocks⁴³⁻⁴⁵ (Fig. 2), all indicate that the platforms supporting these islands

also form part of the Cordillera. Moreover, the mafic volcanic rocks interbedded with the flysch-type sediments on South Georgia demonstrate that the island is a fragment of the Pacific side of the Andean Cordillera rather than a complete cross-section of it, as implied by earlier reconstructions⁵⁻⁷, or part of the Atlantic side, as recently proposed¹². We believe, therefore, that South Georgia originally adjoined Burdwood Bank, close to Isla Navarino with which it has often been compared geologically^{23,24}. Thus the original length of the North Scotia Ridge is unlikely to have been as great as is suggested by earlier reconstructions^{5,6,18}.

The rocks of the South Orkney Islands, principally schists, amphibolites, and other metamorphic rocks, and intensely deformed, possibly late Palaeozoic sediments, are Cordilleran types^{13,16,22}, and the platform along whose northern margin they lie apparently constitutes an extra-Cordilleran Mesozoic-Cenozoic basin like the Magallanes Basin³⁰. A similar basin is located east of the Antarctic Peninsula^{13,16} (Fig. 2), and probably forms much of the submerged platform adjoining the Weddell Sea. The Antarctic Peninsula itself is Cordilleran.

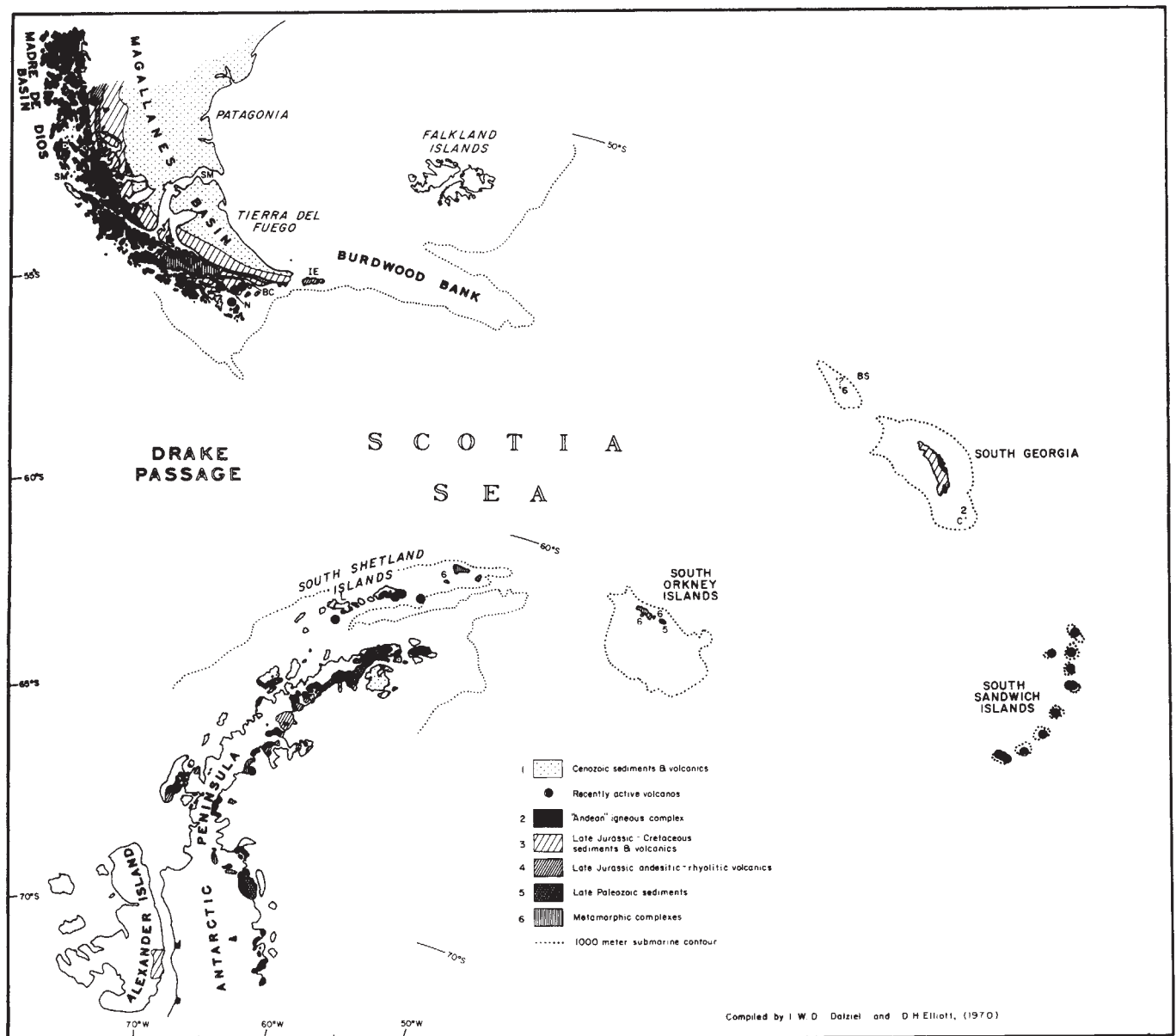


Fig. 2 Geological map of the Scotia Arc region modified after maps by the British Antarctic Survey, the Instituto de Investigaciones Geológicas (Chile), and Dirección Nacional de Geología y Minería (Argentina). BC, Beagle Canal; BS, Black and Shag Rocks; C, Clerke Rocks; IE, Isla de los Estados; L, Livingston Island; N, Isla Navarino.

North of 68° S it consists of "Andean" plutons intruding possibly late Palaeozoic sediments and late Jurassic volcanic rocks²². Irregular distribution of tectono-stratigraphic units results from the mild deformational effects of the Andean Orogeny in this area. The late Mesozoic rocks are therefore essentially flat-lying, and the outcrop pattern is a function of exposure level¹³. Farther south, however, on the Lassiter Coast (75° S, 67° W), deformed Jurassic clastic sediments are intruded by granitic plutons^{47,48} and are analogous to the Andean Cordillera. This may be the case elsewhere south of 68° S in the Antarctic Peninsula, but there is insufficient published data.

Undoubted late Mesozoic sedimentary rocks with interbedded volcanics, comparable with the Pacific side of the Andean High Cordillera, are confined to Livingston Island in the South Shetland group^{49,50} and to Alexander Island⁵¹⁻⁵³ (Fig. 2).

Evolution of the Andean-West Antarctic Cordillera

Southern South America, the North and South Scotia Ridges, and the Antarctic Peninsula are underlain at least in part by continental rocks certainly as old as Palaeozoic^{38,39}, and possibly Pre-Cambrian³⁷. The structure of the basement metamorphic rocks is, however, too complex, and at present too poorly known^{13,15-17,54} for any conclusion to be drawn concerning their relationship to the present Cordillera.

Marine magnetic data (W. C. Pitman, III, personal communication, 1971) and the results of the Deep Sea Drilling Programme⁵⁵ indicate that southern South America and the Antarctic Peninsula formed a marginal strip of Gondwanaland until the late Jurassic. The sedimentary contact between the late Jurassic lavas and the late Palaeozoic sedimentary sequences^{13,17,22,46,54}, together with the parallelism of the major fold axes in the Antarctic Peninsula and Miers Bluff Formation in the South Shetlands with the trend of the peninsula and the South Shetland chain⁵⁴, suggest that the late Palaeozoic sequences were deformed adjacent to Gondwanaland during the early Mesozoic Gondwanian Orogeny. Therefore it is unlikely that the late Palaeozoic sequences were deposited elsewhere and tectonically emplaced adjacent to Gondwanaland.

Deformation of the late Palaeozoic sediments is the first recognizable sign of tectonic instability on the margin of Gondwanaland in the Mesozoic. It occurred not only in what is now the Scotia Arc region, but also in the Ellsworth and Pensacola Mountains of interior West Antarctica, the Cape Fold Belt of southernmost South Africa, and the Sierra de la Ventana of Argentina. In all of these areas (Du Toit's "Samfrau" geosyncline, see Fig. 4a) thick sedimentary deposits were highly deformed in the early Mesozoic (late Triassic-early Jurassic). The deformation was characteristically accompanied by at most low grade metamorphism, which may indicate little underthrusting. On the other hand, the extensive mid to late Jurassic calc-alkaline volcanism following uplift and erosion probably represents the surface evidence of a subduction zone that developed along the margin of South America and West Antarctica, as they began to move westward relative to Africa by spreading of the ocean floor. The most recent estimates of the time of opening of the South Atlantic, based on marine magnetic data (W. C. Pitman, III, personal communication, 1971) and the Deep Sea Drilling Programme⁵⁵, are consistent with this interpretation.

Deformation and uplift along the axis of the present Cordillera began in the early Late Cretaceous in western South America and was pre-mid Cretaceous at the southern end of the Antarctic Peninsula^{47,48}, but did not occur until the latest Cretaceous or earliest Cenozoic in the sub-Andean belt on the Atlantic side of the Cordillera in South America⁵⁶. Pre- and syn-tectonic granitic plutons do occur^{13,15,16,39,40}, and post-tectonic intrusions of latest Cretaceous age^{13,15,16,38-40} are common in the Cordillera. One intrusion as young as 12 m.y. (refs. 38 and 57) occurs in the sub-Andean belt between the

High Cordillera and the extra-Cordilleran basin. The widespread structural effects of the Andean Orogeny on the rocks of the South Scotia Ridge and the northern Antarctic Peninsula were limited to relatively open folding, faulting and intrusion^{13,16,22}.

Evolution of the Scotia Arc

The geology and geological history of the region place the following constraints on the evolution of the Scotia Arc:

(1) The similarity of the geology and geological history in southern South America, the continental fragments bordering the Scotia Sea, and the Antarctic Peninsula make it virtually certain that there was once a continuous Andean-West Antarctic Cordillera.

(2) The right-angle bend in the southernmost Andes, the direction of overfolding and overthrusting there (always at right angles to the Cordillera and from the Pacific side), the fact that South Georgia represents a part of the Pacific side of the Cordillera, and limited palaeomagnetic data from the Antarctic Peninsula^{7,58} all indicate bending, disruption, and eventual fragmentation of the Cordillera.

(3) Dating of the deformational and intrusive events of the Andean Orogeny indicates that the fragmentation cannot have occurred before the end of the Mesozoic. The striking similarity of Mesozoic tectonostratigraphic provinces around the Scotia Basin seems to demand continuity at least until the end of the Andean Orogeny.

(4) The disruption must have occurred prior to the inception of volcanicity in the South Sandwich Ridge, where the oldest dated rocks are 4 m.y. old (ref. 25). But the magnetic data are interpreted^{10,21} to indicate an age of at least 8 m.y. for the South Sandwich Ridge, and that sea floor spreading has occurred from at least mid-Cenozoic (20-25 m.y. ago) until Recent in the Drake Passage. Hence the disruption and fragmentation of the Andean-West Antarctic Cordillera occurred in the early Cenozoic prior to 20-25 m.y. ago, or else accompanied spreading in the Drake Passage. Dott has^{9,59} postulated that the fragmentation was entirely late Cenozoic, accompanying a global late Cenozoic tectonic episode.

Fig. 3 shows our concept of the evolution of the Scotia Arc based on these constraints. The Falkland (Malvinas) Platform is regarded as being a stable part of the South American continent at this time, so that bending of the Andean Cordillera resulted in the creation of the Falkland Trough between the North Scotia Ridge and the Falkland Platform.

Note that elimination of the South American and Antarctic Peninsula "oroclines" with the continents held in their present positions (inset) results in considerable and unacceptable overlap. This suggests that despite the spreading in the Drake Passage indicated by Barker's identification of the magnetic anomalies there^{12,21}, South America and West Antarctica are closer together now than they were when they separated from Gondwanaland (Fig. 3a). Some of the late Cenozoic-Recent spreading in the Drake Passage could have been taken up in subduction zones along the North and South Scotia Ridges. This is likely along the South Scotia Ridge that is bordered on its northern side by deep troughs⁶⁰, and was the site of andesitic volcanism in the early Cenozoic, prior to the onset of mildly alkaline activity at the beginning of the Pliocene^{22,61}. Troughs also occur along the southern margin of the North Scotia Ridge⁵⁹, but the only sign of Cenozoic volcanism there is the presence of a few small vents north and west of Cape Horn.

Even if much of the spreading was not taken up in subduction zones, but rather by movement of the continents, the geology does not allow a cusp-shaped original connexion between the continents as suggested by Barker and Griffiths with the Pacific margin of the Cordillera extending no farther east than Cape Horn and Elephant Island¹² (Fig. 2), and with South Georgia on the Atlantic side. South Georgia is clearly part of the Pacific side of the Cordillera^{13,23,24}. The geological

evidence, however, dictates only a latest Cretaceous maximum age limit for the disruption and fragmentation of an originally near rectilinear Cordillera. It is possible, therefore, that the two continents approached one another more closely than at present during the early Cenozoic. The spreading in the Drake Passage over the past 20 m.y. may document subsequent separation from a cusp-shaped contact if little subduction occurred. Hence the two lines of evidence do not necessarily lead to mutually exclusive models.

The reason for the disruption of the original Cordillera in the Drake Passage is not entirely clear. The predominant factor appears to have been the narrowness of the continental connexion between the two parts of Gondwanaland that are now South America and West Antarctica when the South Atlantic Ocean Basin began to open. In all probability the lithospheric plate underlying the Pacific Ocean tended to bend and disrupt this continental strip rather than continue to be consumed beneath it. The initial bending of the Cordillera may, therefore, have accompanied rather than post-dated the Andean Orogeny. Katz⁶² has pointed out that pronounced virgation of folds in the sub-Andean belt of South America occurs at the hinge of the orocline.

or in a combination of them: (1) anticlockwise rotation of the Antarctic Peninsula relative to East Antarctica following continental separation, (2) right-lateral strike-slip faulting along the margin between East and West Antarctica subsequent to continental separation, (3) a "fit" of East Antarctica and Africa other than those recently proposed^{64,65}.

There is stratigraphic evidence to support the existence of a sedimentary basin with deep water facies in the South Atlantic area in the late Palaeozoic⁶⁵. This basin need not necessarily have been oceanic in character, nor as large as has been proposed (Fig. 4d). Strike-slip displacement and/or rotation of West Antarctica relative to East Antarctica have been proposed elsewhere⁶⁵⁻⁶⁷ and might well explain the anomalous swing in strike and termination of the early Mesozoic fold structures in the Ellsworth Mountains⁶⁸. Finally, the East Antarctica-Africa "fit" seems to have considerable geological and bathymetric uncertainty.

Final solution of the problem must await further work on the magnetic anomalies from the southern oceans, and a better understanding of the different parts of Du Toit's "Samfrau" geosyncline⁶³ (Fig. 4a). Our model, however, demands a solution close to that of Fig. 4e (ref. 67).

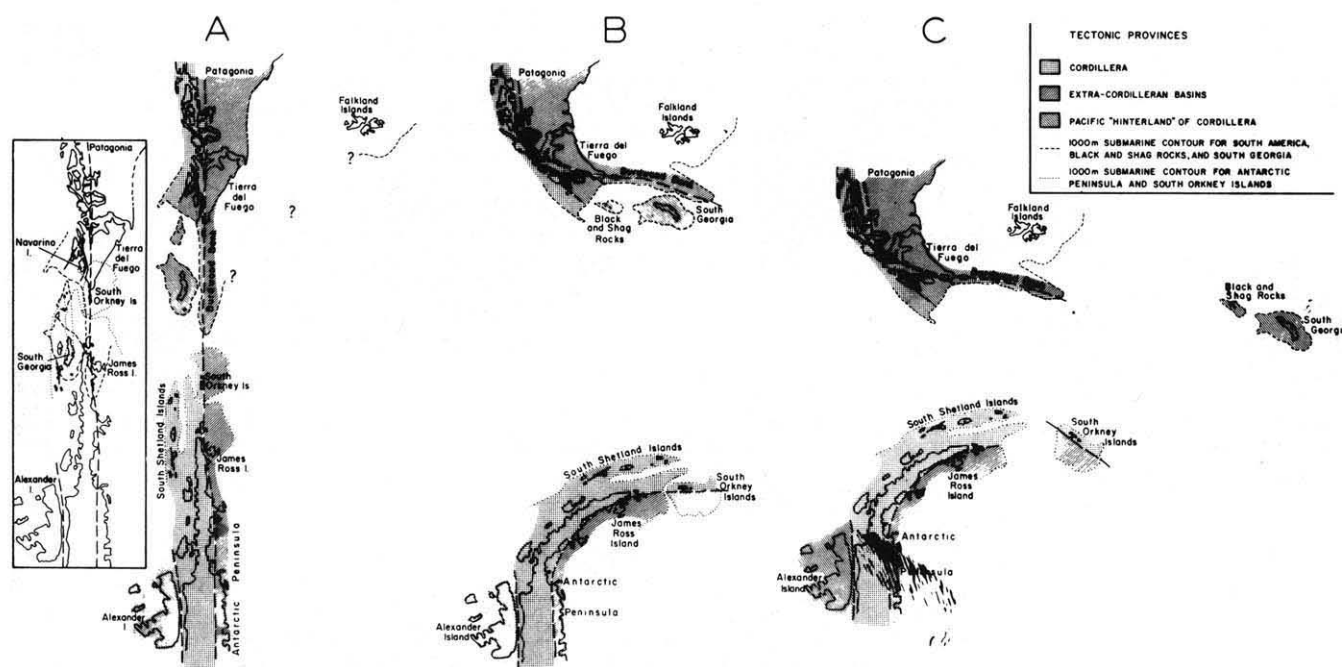


Fig. 3 Evolution of the Scotia Arc. *a*, Andean-West Antarctic Cordillera in the latest Mesozoic following the Andean Orogeny and preceding bending and disruption (insert: restoration of Cordillera assuming no north-south separation of South America and the Antarctic Peninsula). *b*, Scotia Arc in the (?) early Cenozoic following bending and disruption of the Cordillera, but preceding dispersal of continental fragments. *c*, Present distribution of continents and continental fragments around the Scotia Sea.

Reconstruction of Gondwanaland

Attempts to reconstruct the initial configuration of Gondwanaland⁶³⁻⁷⁰ have all encountered difficulty in the area of the Scotia Arc and West Antarctica (Fig. 4). This is probably in part due to the distortion of parts of the continental fragments since their separation from the supercontinent. The main problems in reconstruction have arisen because of unacceptable overlap of the Antarctic Peninsula and Africa⁶⁵ (Fig. 4c), distortion of the Scotia Arc^{64,68}, and overlap of South America and Antarctica⁷⁰.

Our model demands that an essentially rectilinear Cordillera connected South America and West Antarctica until the end of the Mesozoic. This could be reconciled with the generally accepted "fit" of South America and Africa⁷¹ in several ways—

To summarize, there seems little doubt that the southernmost Andes once continued into West Antarctica as an essentially rectilinear Cordillera. A proto-Cordillera developed at latest in the early Mesozoic, and the main Cordillera resulted from the Andean Orogeny in late Mesozoic to early Cenozoic time. Oroclinal bending probably began during the Andean Orogeny, but complete disruption and fragmentation of the Cordillera did not ensue until the Cenozoic. The Pacific margin has been shortened in the vicinity of the Scotia Arc, but only by about 1,000 km. Our model places constraints on the reconstruction of Gondwanaland.

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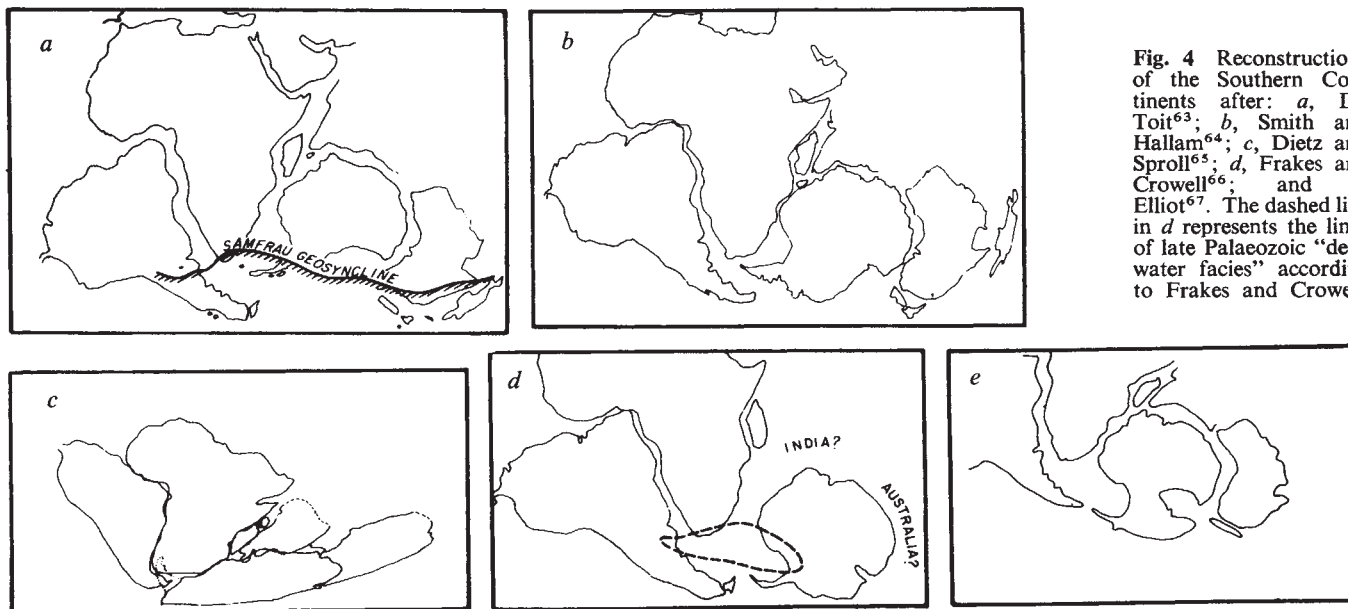


Fig. 4 Reconstructions of the Southern Continents after: a, Du Toit⁶³; b, Smith and Hallam⁶⁴; c, Dietz and Sproll⁶⁵; d, Frakes and Crowell⁶⁶; and e, Elliot⁶⁷. The dashed line in d represents the limit of late Palaeozoic "deep water facies" according to Frakes and Crowell.

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- ¹ Barrow, J., *Royal Geographical Society*, **1**, 62 (1831).
- ² Arctowski, H., *Bulletin de la Société Géologique de la France*, Ser. 3, **23**, 589 (1895).
- ³ Andersson, J. G., *Uppsala University Geological Institute Bulletin*, **7**, 19 (1906).
- ⁴ Herdman, H. F. P., *Report on Soundings taken during the Discovery Investigations, 1926-32*, **6**, 214 (1932).
- ⁵ Matthews, D. H., *Geol. Mag.*, **96**, 425 (1959).
- ⁶ Hawkes, D. D., *Geol. Mag.*, **99**, 85 (1962).
- ⁷ Hamilton, W., *Geological Survey of Canada*, **66-15**, 178 (1966).
- ⁸ Wilson, J. T., *Earth Planet. Sci. Lett.*, **1**, 335 (1966).
- ⁹ Dott, jun., R. H., *Proc. UNESCO-IUGS Symp. on Continental Drift, Montevideo, Uruguay, October 1967* (in the press).
- ¹⁰ Barker, P. F., *Nature*, **228**, 1293 (1970).
- ¹¹ Moores, E., *Nature*, **228**, 837 (1970).
- ¹² Barker, P. F., and Griffiths, D. H., *Phil. Trans. Roy. Soc., London* (in the press).
- ¹³ Dalziel, I. W. D., and Elliot, D. H., in *The Ocean Basins and Continental Margins: 1, The South Atlantic* (edit. by Stehli, F. G., and Nairn, A. E. M.), (Plenum Publishing Corporation, New York, in the press).
- ¹⁴ Dalziel, I. W. D., *Antarctic Journal of the US*, **4**, 137 (1969).
- ¹⁵ Dalziel, I. W. D., *Antarctic Journal of the US*, **5**, 99 (1970).
- ¹⁶ Dalziel, I. W. D., *Antarctic Journal of the US* (in the press).
- ¹⁷ Dalziel, I. W. D., Price, D. P., and Stirewalt, G. L., *Antarctic Journal of the US*, **5**, 100 (1970).
- ¹⁸ Deuser, W. G., *Tectonophysics*, **10**, 391 (1970).
- ¹⁹ Barazangi, M., and Dorman, J., *Seismol. Soc. Amer. Bull.*, **59**, 369 (1969).
- ²⁰ Heirtzler, J. R., *Sci. Amer.*, **219**, 69 (1968).
- ²¹ Barker, P. F., in *Antarctic Geology and Geophysics* (edit. by Adie, R. J.), (Universitetsforlaget, Oslo, in the press).
- ²² Adie, R. J., in *Antarctic Research* (edit. by Priestley, R., Adie, R. J., and de Q. Robin, R.), (Butterworths, London, 1964).
- ²³ Frakes, L. A., *Bull. Geol. Soc. Amer.*, **77**, 1463 (1966).
- ²⁴ Katz, H. R., and Watters, W. A., *New Zealand Journal of Geology and Geophysics*, **9**, 323 (1966).
- ²⁵ Baker, P. E., *Bull. Volc.*, **32**, 189 (1968).
- ²⁶ Ewing, J. I., Ludwig, W. J., Ewing, M., and Eittreim, S. L., *J. Geophys. Res.* (in the press).
- ²⁷ Griffiths, D. H., and Barker, P. F., in *Antarctic Geology and Geophysics* (edit. by Adie, R. J.) (Universitetsforlaget, Oslo, in the press).
- ²⁸ Allen, A., *Brit. Antarct. Surv. Sci. Rep.*, **55** (1966).
- ²⁹ Ludwig, W. J., Ewing, J. I., and Ewing, M., *Amer. Assoc. Petroleum Geologists Bull.*, **52**, 2337 (1968).
- ³⁰ Harrington, P. K., Barker, P. F., and Griffiths, D. H., in *Antarctic Geology and Geophysics* (edit. by Adie, R. J.), (Universitetsforlaget, Oslo, in the press).
- ³¹ Watters, D. G., in *Antarctic Geology and Geophysics* (edit. by Adie, R. J.), (Universitetsforlaget, Oslo, in the press).
- ³² Herron, E. M., and Hayes, D. E., *Earth Planet. Sci. Lett.*, **6**, 77 (1969).
- ³³ Morgan, W. J., and Johnson, G. L., *Trans. Amer. Geophys. Un.*, **51**, 329 (1970).
- ³⁴ Craddock, C., in *Antarctic Geology and Geophysics* (edit. by Adie, R. J.), (Universitetsforlaget, Oslo, in the press).
- ³⁵ Ford, A., in *Antarctic Geology and Geophysics* (edit. by Adie, R. J.), (Universitetsforlaget, Oslo, in the press).
- ³⁶ Tilley, C. E., *Discovery Reports*, **10**, 383 (1935).
- ³⁷ Ilchenko, L. N., in *Antarctic Geology and Geophysics* (edit. by Adie, R. J.), (Universitetsforlaget, Oslo, in the press).
- ³⁸ Halpern, M., *Proc. UNESCO-IUGS Symp. on Continental Drift, Montevideo, Uruguay* (in the press).
- ³⁹ Halpern, M., and Carlin, G. M., *Antarctic Journal of the US* (in the press).
- ⁴⁰ Kranck, E. H., *Acta Geographica Helsingfors*, **4**, 1 (1932).
- ⁴¹ MacFadyen, W. A., *Discovery Reports*, **7**, 1 (1933).
- ⁴² Katz, H. R., *Amer. Assoc. Petroleum Geologists Bull.*, **47**, 506 (1963).
- ⁴³ Tyrell, G. W., *Discovery Reports*, **23**, 37 (1945).
- ⁴⁴ Trendall, A. F., *Falkland Islands Dependencies Survey Sci. Rep.*, **7** (1953).
- ⁴⁵ Trendall, A. F., *Falkland Islands Dependencies Survey Sci. Rep.*, **19** (1959).
- ⁴⁶ Bibby, J. S., *Brit. Antarct. Surv. Sci. Rep.*, **53** (1966).
- ⁴⁷ Laudon, T. S., Lackey, L. L., Quilty, P. G., and Otway, P. M., *American Geographical Society Antarctic Map Folio Series, Folio 12—Geology, Sheet 3, Eastern Ellsworth Land* (1969).
- ⁴⁸ Williams, P. L., Schneider, D. L., Plummer, C. C., and Brown, L. E., in *Antarctic Geology and Geophysics* (edit. by Adie, R. J.), (Universitetsforlaget, Oslo, in the press).
- ⁴⁹ Araya, R., and Hervé, F., *Contribucion del Instituto Antartico Chileno*, **20** (1966).
- ⁵⁰ González-Ferrán, O., Katsui, K., and Taverna, J. J., *Instituto Antartico Chileno, Serie Cientifica*, **17** (1969).
- ⁵¹ Horne, R. R., *Bull. Brit. Antarct. Surv.*, **11**, 1 (1967).
- ⁵² Horne, R. R., *Bull. Brit. Antarct. Surv.*, **17**, 73 (1968).
- ⁵³ Horne, R. R., *Bull. Brit. Antarct. Surv.*, **22**, 61 (1969).
- ⁵⁴ Dalziel, I. W. D., in *Antarctic Geology and Geophysics* (edit. by Adie, R. J.), (Universitetsforlaget, Oslo, in the press).
- ⁵⁵ Maxwell, Q. E., Van Herzen, R. P., Hsu, K. J., Andrews, J. E., Saito, T., Percival, jun., S. F., Milow, E. D., and Boyce, R. E., *Science*, **168**, 1047 (1970).
- ⁵⁶ Katz, H. R., *J. Geol.*, **70**, 595 (1962).
- ⁵⁷ Katz, H. R., *Minerales, Santiago de Chile*, **74**, 1 (1961).
- ⁵⁸ Blundell, D. J., *Brit. Antarct. Surv. Sci. Rep.*, **39** (1962).
- ⁵⁹ Dott, jun., R. H., *Science*, **166**, 874 (1969).
- ⁶⁰ Kroenke, L. W., and Woolard, G. P., *Hawaii Institute of Geophysics Report 68-4* (1968).
- ⁶¹ Baker, P. E., in *Antarctic Geology and Geophysics* (edit. by Adie, R. J.), (Universitetsforlaget, Oslo, in the press).
- ⁶² Katz, H. R., *Geol. Rdsch.*, **54**, 1195 (1964).

- ⁶³ Du Toit, A. L., *Our Wandering Continents* (Oliver and Boyd, Edinburgh, 1937).
- ⁶⁴ Smith, A. G., and Hallam, A., *Nature*, **225**, 139 (1970).
- ⁶⁵ Dietz, R. S., and Sproll, W. P., *Science*, **167**, 1612 (1970).
- ⁶⁶ Frakes, L. A., and Crowell, J. C., *Nature*, **217**, 832 (1968).
- ⁶⁷ Elliot, D. H., in *Antarctic Geology and Geophysics* (edit. by Adie, R. J.), (Universitetsforlaget, Oslo, in the press).
- ⁶⁸ Craddock, C., *American Geographical Society Antarctic Map Series Folio 12—Geology, Sheet 23, Gondwanaland* (1970).
- ⁶⁹ Heirtzler, J. R., in *Antarctic Research* (edit. by Porter, H. D.), (Amer. Assoc. Adv. Science, in the press).
- ⁷⁰ Tarling, D. H., *Nature*, **229**, 17 (1971).
- ⁷¹ Bullard, E. C., Everett, J. E., and Smith, A. G., *Phil. Trans. Roy. Soc. London, A*, **258**, 41 (1965).

Andean Orogeny and Ocean Floor Spreading

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The periodicity and eastward migration of the Andean orogeny is explained in terms of periodic loss of marginal continental crust by crustal subduction.

ALTHOUGH there is strong evidence¹⁻⁴ that new oceanic crust is being created along the oceanic fracture system at rates of several centimetres per year, the evidence that oceanic crust is being destroyed at similar rates in the continental fracture system is less compelling, and some have rejected this hypothesis in favour of the hypothesis of an expanding Earth⁵.

The rate of crustal subduction in the Peru–Chile trench is about 6 cm yr⁻¹ (ref. 6), although changes of spreading rate and direction may have occurred in the past 25 million years^{7,8}. Even so, calculated rates have always been high (5 cm yr⁻¹), and no pauses in spreading have been detected. The undeformed nature of the sediments in the Peru–Chile trench is a problem⁹⁻¹³, however, and so is the disappearance of the probable westward extension of the continental crust of Chile¹⁴. This article gives an account of my view of what has happened, and is based on field studies of the Antofagasta segment in northern Chile (Fig. 1). I suggest that the orogenic history of Chile may be understood in terms of ocean floor spreading if there has been periodic loss of continental crustal material from the continental margin.

The Andes in the Antofagasta segment (Figs. 1 and 2) consist of two main geosynclinal belts, the Cambrian–Devonian East Andean geosyncline and the Jurassic–Cretaceous West Andean geosyncline. On the face of it, this suggests a westward migration of the main trough of sedimentation from the Palaeozoic to the Mesozoic. The West Andean geosyncline, however, was certainly developed on an older continental basement, not on oceanic crust. This basement locally consists of virtually unmetamorphosed Upper Palaeozoic sediments, or more commonly of metamorphic foliated rocks which have usually been regarded as Precambrian but which are probably of Palaeozoic age¹⁵. Thus the Mesozoic geosyncline probably developed on an outer metamorphosed and deformed Palaeozoic geosynclinal belt parallel to the unmetamorphosed inner belt of the East Andean geosyncline. This interpretation is supported by the fact that a belt of Palaeozoic granites and Triassic volcanics lies along the junction of the East and West Andean geosynclines.

The West Andean geosyncline was divided into a “eugeosynclinal” volcanic (coast range) zone and a “miogeosynclinal”

sedimentary (longitudinal valley) zone during the Jurassic (ref. 16, Fig. 5, p. 42), and “infraneritic” and “bathyal” facies have been recognized in the latter zone (ref. 17, Fig. 6, p. 60). The geosyncline was apparently narrower in the Lower Cretaceous and the facies boundary between so-called eugeosynclinal and miogeosynclinal facies was displaced to the east (ref. 16, Parte II, Fig. 4). This was a consequence of the phase of mobility at the end of the Jurassic (chiefly faulting and plutonism) which led to the uplift and erosion of the coast range zone (Fig. 2).

The sea was already excluded from the longitudinal valley zone north of 26° S at the end of the Jurassic and was excluded from the southern part of the West Andean geosyncline before the main phase of mobility at the beginning of the Upper Cretaceous (although marine sedimentation continued still further south in parts of the Magellan geosyncline). Thus it appears that the present difference in elevation between north and south Chile reflects a difference in crustal structure which dates from at least Jurassic time.

Folding in the West Andean geosyncline began with a Cenomanian phase and was continued by later Cretaceous and early Tertiary phases. The Cretaceous folding was not accompanied by regional metamorphism, nor was it characterized by large strains or cleavage development. The folds are generally of large wavelength and amplitude and open in style. In the later phases folding was more localized and generally less intense, giving way to block faulting (with major reverse faults) and acid volcanism. Thick deposits of Upper Cretaceous continental sediments were accumulated in the longitudinal valley zone of the Western Andes in the Upper Cretaceous. The Puna zone also began to develop as a basin and has a thicker Cretaceous sequence than the East Andean zone. Marine incursions in the East Andean zone appear to correspond to the Middle Cretaceous tectonism of the Western Andes.

Following further tectonism in late Cretaceous or early Tertiary time, the Western and Eastern Andes became positive areas subject to repeated uplift and erosion while the Puna and Sub-Andean zones became syn and post-orogenic basins in which the later phases of mobility were concentrated. The western margin of the Puna zone became the site of the main ignimbritic volcanism of the late Miocene and Pliocene and then of the Plio-Pleistocene central volcanoes. The present longitudinal valley and associated topographic features did not develop as basins until Miocene time, when the main uplift of the Puna zone began. Possibly the Peru–Chile trench is of the same age, as discussed later¹³.

There is a tendency for the site of the most intense deformation to migrate eastwards with time. Thus the most intense

deformation in the coast range zone was at the end of the Jurassic, in the longitudinal valley zone during the Upper Cretaceous and in the Sub-Andean zone during the Pliocene. The Puna zone, however, is similar to the Sub-Andean zone and probably had its main deformation after the East Andean zone. The crustal shortening associated with the orogenic deformations is not easily estimated. If the open folding of concentric style is entirely the consequence of crustal shortening and if some allowance is made for the effect of reversed faulting, the total shortening since mid-Cretaceous time might be as much as 40%. The maximum width of the belt is about 700 km, so that the maximum crustal shortening would seem to be 500 km. The actual amount might be much less, and is therefore at least an order of magnitude less than the amount of crust postulated to have been consumed by crustal subduction in the same period.

Plutonic and volcanic activity also migrated from west to east. Upper Jurassic plutons lie on the eastern margin of the coast range zone, with Upper Cretaceous plutons immediately to the east and scattered smaller Tertiary intrusions further east again. The main volcanism was in the coast range zone in the Jurassic, in the longitudinal valley zone in the Cretaceous and early Tertiary, and in the Puna zone in the later Tertiary.

The present coast of north Chile seems to be a fault scarp modified by marine erosion in Plio-Pleistocene time. The steep coastal cliffs are usually several hundred metres high and display a Jurassic lava sequence dipping gently inland. Clearly the Jurassic volcanic zone originally extended considerably further west. Moreover there is no Mesozoic or Tertiary low temperature, high pressure schist belt in Chile like the Franciscan belt of California or those of island arcs. Only a low pressure, high temperature belt is present, so that if paired metamorphic belts were developed the outer belt either lies offshore or has disappeared. Several mechanisms for the removal of marginal continental crust have been put forward^{9,14}, and it seems possible that the missing crust has been dragged under the continent where it has been remobilized and has thickened the crust by processes of plutonism and volcanism. The crust in this part of the Andes seems to be thicker than in western North America, for example, where little or no marginal crust has disappeared¹⁷.

The disappearance of marginal crust in this way suggests an explanation for the periodicity and spatial migration of Andean plutonism and volcanism. If the Benioff zone maintains a roughly constant attitude through time, then the site of volcanic and plutonic activity should be a roughly constant distance from the subduction zone. The periodic eastward migration of plutonism and volcanism is then a consequence of periodic loss of marginal continental crust in the subduction zone. The site of Jurassic volcanism was roughly 200 km west of the present site, so it may be supposed that about 200 km of continental crust lay west of the present coast at the beginning of the Jurassic. The three principal phases of granitic plutonism at the end of the Jurassic, in Upper Cretaceous and in Tertiary time may represent three stages in the destruction of the marginal continental crust. The width of the main fault blocks of the present day Chilean Andes (100 km) suggest that each phase of volcanic and plutonic activity is related to the subduction of one fault block.

This hypothesis also suggests that there should be some correlation between tectonism and plutonism. A phase of compressional tectonics causing the subduction of a strip of marginal continental crust should precede each phase of plutonism and a phase of isostatic uplift should accompany the plutonism. This appears to hold for the history of plutonism and tectonism in Chile.

The theory of crustal subduction, including subduction of continental crust, therefore seems to provide a satisfactory model for the development of the Andes of northern Chile, and adjacent Argentina and Bolivia. The chief difficulties, however, are the undeformed nature and the volume of the sediments in the Peru-Chile trench⁸⁻¹³. Scholl *et al.*¹³ have

concluded that: (1) Sediment removal probably occurred at the base of the continental margin and adjacent deep-sea floor in late Cretaceous and perhaps earliest Tertiary time. (2) Uncertainties in denudation-sedimentation budget leave open the question of tectonic removal of sediment in Tertiary time. (3) Reasonable estimates of erosion suggest that late Cenozoic underthrusting at the postulated rate has not taken place or that it has taken place without removing sediment from the trench. They are driven to suggest that sinking of the plate may take place seaward of the trench.

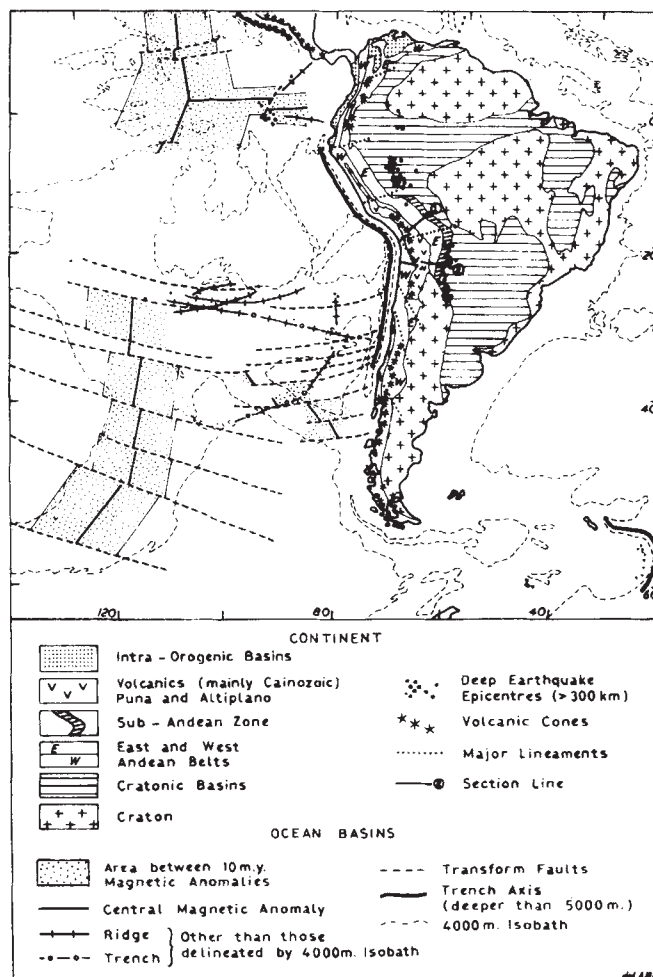


Fig. 1 Major tectonic zones of the Andes of South America in relation to major tectonic and topographic features of the East Pacific. Note that the continuity of the spreading system of the East Pacific Rise is well established but a portion has been omitted from the map for lack of available data.

The hypothesis of subduction of marginal continental crust effectively explains the absence of Mesozoic and early Tertiary sediments. The third point is therefore the most crucial. It is partly based on the inference that most of the fill of the trench south of 36° S consists of turbidites deposited in glacial episodes of lowered sea level during the past three million years and that the volume of these sediments is close to their maximum estimate for this period. Yet, on the basis of the present configuration of the trench, the rate of removal of sediment from the trench by crustal subduction should be greater than the rate of supply. For this reason too, the problem becomes more severe if the trench fill is assumed to be of greater age.

The only alternative explanation that suggests itself to me is that denudation rates on land are not directly relevant to the rate of accumulation in the trench, but that rapid submarine

erosion of the onshore flanks of the trench is responsible for the fill.

The present relief of the Andes has developed largely in the past ten or possibly twenty million years, and it seems probable that the present submarine relief is partly of the same age. Prior to the development of this relief large quantities of sediment may have accumulated on the onshore side of the subduction zone. As the relief of the onshore flank developed, much of this sediment, with the help of earthquake activity, would become unstable and be transported rapidly into the trench. In this way the erosional products of some tens of millions of years might be transported into the trench in a fraction of that time.

for the loss of marginal blocks by crustal subduction although the rate of movement required would be extremely high compared with the observed rates onshore.

The block faulting was initiated in the early Tertiary. In the Antofagasta segment²¹⁻²³ filling of the intermontane troughs began in the Oligocene or Miocene with the Tambores, San Pedro and Lower Loa Formations. Intermittent tectonism in the east of the segment then accompanied the deposition of the Pliocene San Bartolo and Upper Loa Formations.

The Loa Formation (probably the Upper part only) representing an ancestral valley of the River Loa is now preserved at a height of about 400 m in the coastal scarp a few kilometres north of the present gorge of the River Loa. Clearly therefore

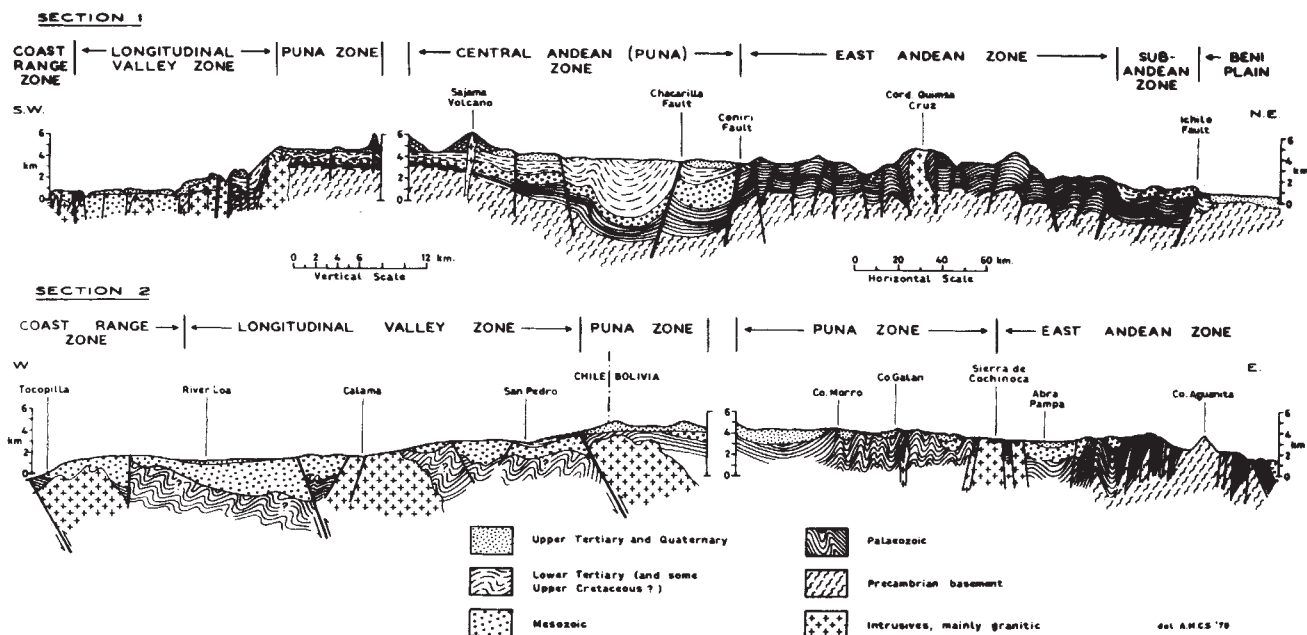


Fig. 2 Sections across the Andean orogenic belt. Section lines are shown on Fig. 1. Section 1 runs north-east from Arica in Chile. The south-western portion is after Salas *et al.* in ref. 16. The north-eastern portion is after Ahlfeld²⁷. Section 2 is across the Chilean and Argentinian Andes near 22° S. The eastern portion is interpreted after Turner²⁸.

The study by Scholl *et al.*¹³ did not extend south of 44° S but earlier studies have shown a very different sediment distribution south of the Chile Rise where seismic evidence suggests that crustal subduction has ceased¹ (although more recent studies have tentatively suggested that subduction is continuing at a reduced rate¹⁸). Sedimentary accumulation is not confined to the southern continuation of the trench but extends westwards for 200 km (ref. 19). The absence of such a wide belt north of the Chile Rise is itself strong evidence for crustal subduction there.

The volume of the sediments in the trench may not therefore mean that crustal subduction is not taking place. Further evidence on the nature of sediments in the trench and their undeformed character comes from an analogy between the trench and onshore fault blocks, which will now be developed.

Contrary to some statements²⁰ which have been quoted recently¹⁹, the fault-blocks of northern Chile are certainly a consequence of compressive tectonics and are limited by reverse faults²¹⁻²³. These faults are exposed, for example, in gorges on the east side of the Pampa de Tamarugal and on the road from Calama to San Pedro de Atacama on the west side of the Cerros de Purilactis. In both cases the faults dip steeply to the east. A similar fault may lie offshore and may be responsible for the coastal scarp, and yet another may occur in the trench. Movement along such faults may well account

the Loa Formation extended further west and probably continued into the ocean before the uplift of the coast range.

These relations suggest that the Peru-Chile trench is at least as old as the block faulted onshore basins and that the surface of acoustic basement is likely to be the unconformity developed across folded Cretaceous and older rocks. In this case four kinds of fill might be expected in the trench or in sediment traps on its onshore flank in the Antofagasta segment: (1) products of dissection of the block-faulted terrain prior to deposition of the Loa Formation, (2) equivalents of the Loa Formation carried into the ocean by the ancestral River Loa, (3) sediment carried to the ocean during the period when the present River Loa gorge was cut, and (4) material removed from the coastal fault scarp by marine erosion.

The evidence of points (1), (2) and (3) demonstrates that the present climatic zonation has not controlled offshore sedimentation through most of the Tertiary. The large volumes of coarse piedmont deposits of pre-Pliocene age in N. Chile shows that discharge and erosion from the high Andes was sometimes much higher than at present. Periods of piedmont aggradation have alternated with periods of dissection^{24,25}. The estimates of denudation rates used by Scholl *et al.*¹³ are therefore questionable. If denudation rates were in fact higher than their estimates, their data on sediment volumes suggest that insufficient Tertiary sediment is now to be found

offshore in N. Chile. Consumption of sediment in the Benioff zone or its conversion to acoustic basement may therefore have occurred in the Tertiary as required by the hypothesis of ocean floor spreading.

The onshore intermontane basins show that reversed faulting of their eastern margins has proceeded since the Miocene without deformation of their sedimentary fill. If such reversed faults are utilized for the crustal subduction process it seems quite possible that the process could proceed without folding of the trench sediments. The subsidence of the oceanic crust beneath the prow of continental crust can probably be achieved by reversed faults conjugate to the main easterly dipping reversed fault. If normal faults are commonly developed by the downbending of the oceanic crust^{24,25} on the offshore flank of the trench, these normal faults would probably be utilized as the conjugate reversed faults as the oceanic crust passes into the Benioff zone beneath the continental flank of the trench. I therefore conclude that although the postulated rate of subduction of oceanic crust is startling in comparison with processes in the adjacent continental crust, the known geology of the trench and adjacent continental margin is not incompatible with the theory of crustal subduction.

The application of the theory of ocean floor spreading, and in particular of crustal subduction therefore offers a possible explanation of the periodicity within the West Andean orogenic cycle and of why sedimentary facies boundaries, deformation, volcanism, and plutonism should have migrated eastwards. Belts in which there has been no loss of the continental margin or in which continental accretion has occurred should show distinctly different relations. It also seems possible that the extremely high percentage of volcanic rocks in the Andean geosyncline and the nonmarine character of the geosyncline during much of its history may be a consequence of isostatic effects resulting from subduction of marginal continental crust.

Studies of the development of the Chile rise in relation to South America and comparative studies of the Andes on either side of the rise clearly provide a fruitful field for further research on the relation of ocean floor spreading and orogenic processes. A superficial study of the geology and of palaeogeographic maps¹⁶ does not reveal the development of startling contrasts in the Tertiary when the Chile Rise, presumably moving north from Antarctica, may have begun to prevent the easterly flow of oceanic crust beneath southern Chile. Young volcanoes, for example, are developed well south of the Chile Ridge in the Andes just as they are further north although petrological differences have been noted. The limitation of the Peru-Chile trench by the Galapagos and Chile ridges is striking, however, and current seismicity appears to be similarly controlled.

In this connexion it may be noted that the concave disposition of the seismic zone towards the Pacific appears to be related to its dip. North and south of the sub-Andean zone (Fig. 1) the epicentres of deep focus earthquakes have colinear distributions at a substantial angle to the orogenic trend. The overall trend of the epicentres is NNW approximately normal to the major ENE lineaments crossing the orogenic belt. Thus it appears that the dip of the seismic zone is at a maximum at the apex of the arc. The symmetry of the pattern also suggests that movements in the subduction zone are in the ENE direction parallel to the major lineaments. It may also be noted that the bathymetric maps of the Pacific reveal a ridge approximately parallel to the orogenic belt, the significance of which is unknown.

The application to the Andean orogenic belt of the hypothesis of ocean floor spreading as developed here does not explain the progressive change in the orogenic belt as a whole. Analogy with older orogenic belts shows that the orogenic cycle is virtually complete but the implications of this for ocean floor spreading are not clear. Perhaps the cycle comes to an end when indigestion in the subduction zone can no longer be relieved by lateral migration of the geosyncline.

But the Mesozoic-Tertiary orogenic cycle shows similar

broad features in terms of geosyncline development and subsequent uplift in all areas irrespective of whether the continental margin has been destroyed as in Chile, created as in the Philippines, or is marked by transform faulting as in California. These features are found not only on present continental margins but also in the Alps and Himalayas where two areas of continental crust have been brought together. Rejuvenation of relief in Neogene times also characterizes older Phanerozoic and Late Precambrian orogenic belts. Attempts²⁶ to explain orogeny in general and vertical movements in particular in terms of constant rate ocean floor spreading therefore appear inadequate at present.

My conclusions are as follows: (1) The Andes of Northern Chile and adjacent Argentina and Bolivia display a periodic eastward migration of volcanism, plutonism and tectonism which can be explained by periodic subduction of blocks of marginal continental crust. Thus the Jurassic coastline was probably 200 km west of the present coast. (2) The hypothesis explains the absence of Mesozoic and Tertiary sediments offshore. (3) The volume and undeformed nature of the young sediments in the Peru-Chile trench provide obstacles to the easy acceptance of the crustal subduction hypothesis. These obstacles may be overcome if rapid submarine denudation of the onshore flank of the trench is accepted along with the analogy between the onshore fault basins and the trench. (4) Further work on the relations north and south of the Chile Rise is needed to relate orogenic processes to the postulated differences in rates of crustal subduction. (5) The late Cenozoic vertical movements in orogenic belts may not be directly dependent on ocean floor spreading.

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- ¹ Isacks, B., Oliver, J., and Sykes, L. R., *J. Geophys. Res.*, **73**, 5855 (1968).
- ² Heirtzler, J. R., Dickson, G. O., Herron, E. M., Pitman, W. C., and Le Pichon, X., *J. Geophys. Res.*, **73**, 2119 (1968).
- ³ McManus, D. A., and Burns, R. E., *Ocean Industry*, **4**, 40 (1969).
- ⁴ Maxwell, A. E., and Von Herzen, R. P., *Ocean Industry*, **4**, 64 (1969).
- ⁵ Rickard, M. J., *Tectonophysics* (in the press).
- ⁶ Le Pichon, X., *J. Geophys. Res.*, **73**, 3661 (1968).
- ⁷ Morgan, W. J., Vogt, P. R., and Falls, D. R., *Nature*, **222**, 137 (1969).
- ⁸ Herron, E. M., and Hayes, D. E., *Earth Planet. Sci. Lett.*, **6**, 77 (1969).
- ⁹ Scholl, D. W., von Huene, R., and Ridlon, J. B., *Science*, **159**, 869 (1968).
- ¹⁰ Seyfert, C. K., *Nature*, **222**, 70 (1969).
- ¹¹ Lomnitz, C., *Nature*, **222**, 366 (1969).
- ¹² Malahoff, A., *J. Geophys. Res.*, **15**, 1992 (1970).
- ¹³ Scholl, D. W., Christensen, M. N., von Huene, R., and Marlow, M. S., *Geol. Soc. Amer. Bull.*, **81**, 1339 (1970).
- ¹⁴ Miller, H., *Geol. Rundschau*, **59**, 927 (1970).
- ¹⁵ Gonzalez-Bonorino, F., and Aguirre, L., *Geol. Rundschau*, **59**, 979 (1970).
- ¹⁶ Ruiz, F. C., *Geologia y Yacimientos metaliferos de Chile* (Inst. Invest. Geol., Santiago, Chile, 1965).
- ¹⁷ Biese, W., *Inst. de Geologia, Universidad de Chile, Pub.* **19** (1961).
- ¹⁸ Barker, P. F., *Nature*, **228**, 1293 (1970).
- ¹⁹ Hayes, D. E., *Marine Geol.*, **4**, 309 (1966).
- ²⁰ Oppenheim, V., *Amer. J. Sci.*, **245**, 158 (1947).
- ²¹ Hollingworth, S. E., *Nature*, **201**, 17 (1964).
- ²² Rutland, R. W. R., *Int. Council Sci. Unions, Upper Mantle Project, Second Aust. Progr. Rep.* 1965/67 (Canberra, 1967).
- ²³ Thomas, N. A., *Geol. Rundschau*, **59**, 1013 (1970).
- ²⁴ Sillitoe, R. H., Mortimer, C., and Clark, A. H., *Trans. Instn. Min. Metall. (Sect. B)*, **77**, B166 (1968).
- ²⁵ Henriquez, A. H., *Geologia y Yacimientos Metaliferos de Chile: Sedimentos Cuaternarios* (Inst. Invest. Geol., Santiago, Chile, 1965).
- ²⁶ Coney, P. J., *Geol. Soc. Amer. Bull.*, **81**, 739 (1970).
- ²⁷ Ahlfeld, F., *Geol. Rundschau*, **59** (1970).
- ²⁸ Turner, J. C. M., *Geol. Rundschau*, **59**, 1028 (1970).

LETTERS TO NATURE

PHYSICAL SCIENCES

Variations in the 10 μm Flux from NGC 1068

ALTHOUGH there is considerable evidence of rapid variability of the intense infrared sources in the nuclei of galaxies and quasars, conclusive data have been lacking. NGC 1068 is one of the most powerful of the extragalactic infrared sources ($L_{\text{IR}} = 6 \times 10^{45} \text{ erg s}^{-1}$) and, with the exception of M82 which is extended, has the highest signal-to-noise ratio at 10 μm . We have therefore monitored NGC 1068 for several years to look for variations. The pertinent observations are summarized in Fig. 1, where data obtained during the past 3 yr have been combined into four points. The results of the individual observations are given in Table 1.

Table 1 Observations of NGC 1068 at 10 μm

Julian date	Flux level ($10^{-26} \text{ W m}^{-2} \text{ Hz}$)	Standard deviation ($10^{-26} \text{ W m}^{-2} \text{ Hz}$)	Field of view (arc s)
2440167	20.9	1.7	20
2440171	26.8	3.0	20
2440230	25.1	2.5	20
2440509	28.1	1.2	6
2440568	25.2	1.1	6
2440594	26.2	1.3	6
2440868	21.1	2.5	20
2440873	23.0	1.6	20
2440882	25.5	1.4	20
2440889	22.9	1.6	6
2440910	24.6	1.7	6
2440963	18.6	1.2	6
2440993	18.5	1.9	20
2440996	17.9	0.9	6
2440997	17.2	1.0	6
2441033	20.3	1.1	6

The error bars used in Fig. 1 and throughout this discussion are to be interpreted as one standard deviation. They include statistical errors determined from an analysis of the data for each observation and an overall estimate of non-statistical errors determined from the non-repeatability of observations of bright stars. Thus they include all the known errors of observation except an uncertainty of 15% in the absolute calibration of the photometric system. Our discussion is concerned only with measures relative to certain standard stars and so the absolute calibration is not relevant.

The older data on NGC 1068 (refs. 1, 2) have been reanalysed to insure a uniform reduction procedure and to refer all observations to the same standard star, α Tau. Reference to α Tau could usually be made directly but for cases in which secondary standards were used, the estimated errors were increased accordingly. Our observations show that α Tau has remained constant relative to other standard stars to within 4% during this time. The observations were also carefully re-examined; all data for which there is reasonable

doubt about the standardization or observational procedure have been rejected.

The evidence for variability is considerably strengthened by the observations since Julian Date 2440950. Most of these data were obtained with a new photometer; it is important, therefore, to examine whether the observations were affected by the change in instrumentation. We know of only two ways in which this change could affect our results: a shift in the filter passband, and, if the source is polarized, the polarizing effect of a 45° diagonal mirror used to deflect the radiation onto the detector. The filters used in the two photometers have been examined carefully and the only significant difference is that the new filter cuts on at about 7.6 μm whereas the old one cuts on near 7.8 μm . According to unpublished data, the atmospheric transmission from 7.6 to 7.8 μm at our site averages less than 15%. From these measurements, we can place a conservative upper limit of 2% on the effect of the change in filters on our observations. The polarizing effect of a front-aluminized, 45° diagonal mirror at 10 μm is less than 1% (H. M. Dyck, private communication). To make sure that the change in instrumentation does not influence our conclusions, all flux levels from NGC 1068 observed with the new photometer have been increased by 3%, the combined upper limit to the two effects.

The conclusion that variations have occurred does not depend solely on the data obtained with the new instrument. There is evidence for variability in the data obtained before JD 2440950. In addition, the measurement on JD 2440993 was made with the old instrument and is in excellent agreement with other measurements made at about the same time. This observation alone differs from the mean of the observations before JD 2440950 by 3.3 standard deviations. The work on JD 2440910 was done with the same filter used in the new photometer; it differs from the mean of the measurements made since JD 2440950 by 3.5 standard deviations.

Most kinds of systematic error would appear equally in observations of all objects with similar spectra. For this reason, it is customary to present observations of a comparison star together with evidence for variations of an astronomical object. Such a procedure is, however, meaningless in this case, because the spectra of stars and galactic nuclei are quite different at 10 μm . The only reasonable comparison object is another galactic nucleus. Sufficient data to permit such a comparison are available for NGC 1275 and NGC 4151 (refs. 1, 3) although the ratio of signal to noise is much lower for these objects than for NGC 1068; the data are shown in Fig. 1. A detailed analysis of these data shows that the fluxes measured with our new photometer average 0.98 ± 0.08 times those measured with the old one, a result which tends to confirm our conclusion that the observed variation in NGC 1068 is not an instrumental effect.

NGC 1068 and NGC 1275 have frequently been observed on the same nights. It is therefore possible to compare these galaxies directly, without any reference to standard stars. Such a comparison should be independent of most kinds of systematic error. Data are available for four nights near JD 2440550 and for three nights near JD 2441000. They show that the flux from NGC 1068 decreased by $30 \pm 8\%$ relative to that from NGC 1275 during this period of time.

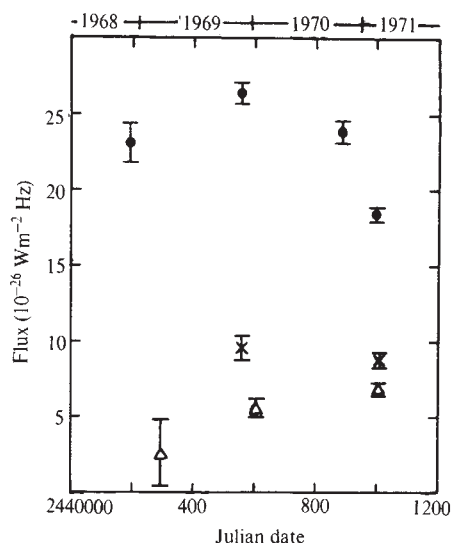


Fig. 1 Variation in the 10 μm flux from the nucleus of NGC 1068. The error bars include all known sources of error except for an uncertainty in the absolute calibration; they are to be interpreted as one standard deviation. The observations of NGC 1275 and NGC 4151, which are included as comparison objects, have been arbitrarily renormalized. ●, NGC 1068; ×, NGC 1275; Δ, NGC 4151.

Similar but larger variations during an earlier period of time have been described for 3C273 and NGC 4151 (ref. 1). The evidence for these variations was not conclusive because the observations had a low signal-to-noise ratio and were made in conditions which made a detailed study of the errors impossible. Neither of these problems affects the data on NGC 1068; the evidence of variation is compelling. The variability of NGC 1068, combined with the similarities between it and NGC 4151 and 3C273 in other respects, greatly strengthens the argument that these sources also vary.

Although the most complete observations have been made at 10 μm , it seems that the 5 μm flux from NGC 1068 has also decreased. Before JD 2440800, the average flux at this wavelength was $7.3 \pm 0.6 \times 10^{-26} \text{ W m}^{-2} \text{ Hz}$; since then, a level of $5.5 \pm 0.6 \times 10^{-26} \text{ W m}^{-2} \text{ Hz}$ has been observed. Our observations are consistent with a similar variation at 3.5 μm . At this wavelength, the observed fluxes are $2.9 \pm 0.5 \times 10^{-26} \text{ W m}^{-2} \text{ Hz}$ before JD 2440800 and $2.4 \pm 0.2 \times 10^{-26} \text{ W m}^{-2} \text{ Hz}$ since then. At 2.2 μm , the flux from the central region of NGC 1068 is predominantly due to stars. Observations of variations in the nuclear source would be extremely difficult at this wavelength².

Terrell⁴ has considered the limits which can be placed on the sizes of astronomical objects from observations of variations in flux. Sizes considerably greater than cT (where c is the speed of light and T is the time scale of the fluctuations) are possible if the source consists of a large, optically thick sphere which re-radiates the emission from a varying central source. An optically thick shell with a diameter of the order of 3 pc and containing dust at a temperature of about 400 K would be consistent with our observations of the nucleus of NGC 1068, although the spectrum at wavelengths longer than 15 μm would have to be attributed to a larger, cooler component.

More generally, if the varying source is not optically thick or is not of an appropriate shape, the upper limit on its diameter would be of the order of $cT = 0.3 \text{ pc}$. The source would have to radiate at least $7 \times 10^{-26} \text{ W m}^{-2} \text{ Hz}$ at 10 μm , the amount of the observed variation. It would have insufficient surface area to radiate this level of flux thermally unless its temperature were greater than 1200 K. A temperature of this order is acceptable only if the emissivity of the radiating dust grains decreases rapidly towards shorter wavelengths so

that the estimated flux agrees with that observed at 2.2, 3.5 and 5 μm .

The observations reported here thus place strong constraints on models for the generation of the far infrared flux from NGC 1068 by thermal means. Many other galactic nuclei also radiate strongly in the infrared^{1,3}; the similarity of their spectra to that of NGC 1068 suggests that these constraints hold for them also.

Radio and optical variability have already been observed in the nuclei of Seyfert galaxies⁵⁻⁷ such as NGC 1068. The fluxes modulated in these regions of the spectrum are, however, relatively small when compared with the total emission of the nuclei, most of which is in the infrared⁸. The observations reported here are the first strong evidence that a significant proportion of the nuclear emission is modulated. In the 10 μm atmospheric window alone, we have observed a change in luminosity of $2.5 \times 10^{43} \text{ erg s}^{-1}$ which is comparable with the total luminosity of our galaxy and about 0.5% of the total luminosity of the nucleus of NGC 1068. It can be inferred that the total change in luminosity was much greater when allowance is made for unobserved regions in the infrared spectrum.

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- Kleinmann, D. E., and Low, F. J., *Astrophys. J. Lett.*, **159**, L165 (1970).
- Garmire, G., Neugebauer, G., Rieke, G. H., and Low, F. J., *Astrophys. J. Lett.*, **166**, L45 (1971).
- Kleinmann, D. E., and Low, F. J., *Astrophys. J. Lett.*, **161**, L203 (1970).
- Terrell, J., *Astrophys. J.*, **147**, 827 (1967).
- Fogarty, W. G., Epstein, E. E., Montgomery, J. W., and Dworetzky, M. M., *Astron. J.*, **76**, 537 (1971).
- Fitch, W. S., Pacholczyk, A. G., and Weymann, R. J., *Astrophys. J. Lett.*, **150**, L67 (1967).
- Zaitseva, G. V., and Lyutiy, V. M., *Astron. Zh.*, **46**, 237 (1969).
- Low, F. J., and Aumann, H. H., *Astrophys. J. Lett.*, **162**, L79 (1970).

Comments on the Evolution of the North-East Atlantic

It has been proposed¹⁻³ that the opening of the Bay of Biscay occurred by rotation about a pole situated near Paris (50.0° N, 3.3° E) and not about the pole proposed by Bullard *et al.*⁴ (43.6° N, 1.0° E). The rotation about the first pole results in strike-slip motion along the North Pyrenean Fault during the pre-Upper-Cretaceous opening, while the rotation about the second pole results in compression there. The second rotation is not compatible with geological data⁵ which indicates extension prior to Upper Cretaceous in the Pyrenean region.

We wish to show how these two hypothetical rotations agree with the magnetic data of Williams and McKenzie⁶.

Fig. 1 shows a rotation of the Iberian peninsula (contour at the 1,000 fathom isobath) by 23° about the first pole. Intermediary rotated positions every 5° are also shown. Similarly, Fig. 2 shows a rotation of the Iberian peninsula by 27.8° about the Bullard *et al.* pole. If the 1,000 fathom contour represents the limit of the continent, most of the area between anomaly 32 and the western limit of Portugal (except for the southernmost part presumably related to movement of Africa) would have been created by the relative rotation of the Iberian peninsula in Fig. 1, but not in Fig. 2. The first rotation would thus seem to be in better agreement with Williams and McKenzie's data. In addition, this first rotation explains the shape of anomaly 32 as reflecting the pre-drift position of the Iberian peninsula.

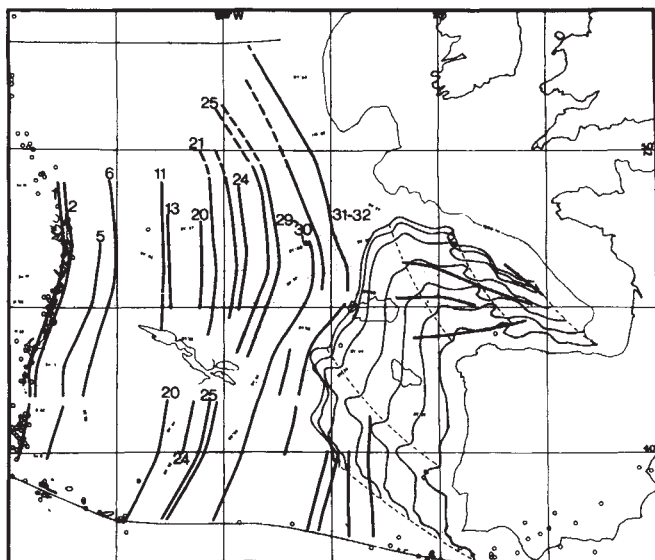


Fig. 1 Magnetic anomalies after Williams and McKenzie⁶. The Iberian Peninsula has been rotated about a pole at 50.0° N, 3.3° E. Several flow-lines are shown in dashed lines.

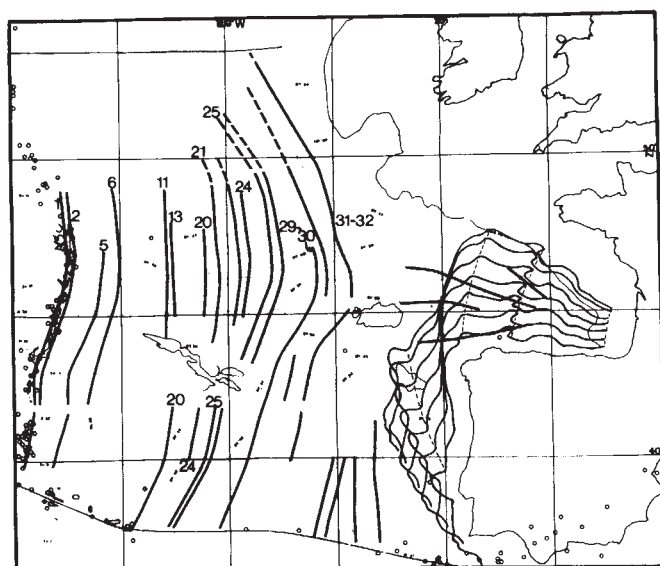


Fig. 2 Magnetic anomalies after Williams and McKenzie⁶. The Iberian Peninsula has been rotated about the Bullard *et al.* pole at 43.6° N, 1.0° E. Several flow-lines are shown in dashed lines.

But even in Fig. 1 the detailed agreement between measured and predicted magnetic anomalies is not good. This may result from inaccurate identification of the anomalies east of 32 by Williams and McKenzie, as a detailed 10 km spaced systematic aeromagnetic survey of the Bay of Biscay (ref. 7 and unpublished work of E. Le Borgne, J. Le Mouél and X. Le P.) has shown the anomalies there to be less fan-shaped than indicated by their map. In any case, an extension toward the west of the systematic detailed survey would enable a definite choice to be made between these two possible rotations.

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- ¹ Le Pichon, X., Bonnin, J., and Sibuet, J. C., *CR Acad. Sci.*, **271**, 1941 (1970).
- ² Le Pichon, X., Bonnin, J., Francheteau, J., and Sibuet, J. C., Une hypothèse d'évolution tectonique du golfe de Gascogne. Symp. sur l'histoire structurale du golfe de Gascogne, Rueil-Malmaison (in the press).
- ³ Le Pichon, X., and Sibuet, J. C., *Earth Planet. Sci. Lett.* (in the press).
- ⁴ Bullard, E. C., Everett, J. E., and Smith, A. G., *Phil. Trans. Roy. Soc., A*, **258**, 41 (1965).
- ⁵ Mattauer, M., *Earth Planet. Sci. Lett.*, **7**, 87 (1969).
- ⁶ Williams, C. A., and McKenzie, D., *Nature*, **232**, 168 (1971).
- ⁷ Le Borgne, E., and Le Mouél, J., *CR Acad. Sci.*, **271**, 1167 (1970).

Implied Superheavy Element Decay Lifetime from Meteorites

Schramm and Fowler¹ demonstrated that superheavy elements could be synthesized in the *r*-process. In the light of these calculations it is interesting to look at the anomalous heavy Xe isotopic composition of carbonaceous chondrites. Rowe² has shown that these isotopic anomalies could be caused by the fission of some unknown nucleus. The Xe isotopic composition for this carbonaceous chondrite fission (CCF) spectrum is very different from the ²⁴⁴Pu fission spectrum measured by Alexander *et al.*³ and observed in other types of meteorites⁴⁻⁶. A reasonable candidate for this unknown fissioning nucleus would be a long lived element⁷⁻¹⁰.

Assuming CCF is due to the fission of a superheavy element (SH) it is possible to estimate the half life of SH using the production rates calculated by Schramm and Fowler¹ and the interval, Δ , between the termination of nucleosynthesis and the time of Xe gas retention. The value of Δ has been estimated^{11,12} from ¹²⁹I-¹²⁹Xe and ²⁴⁴Pu-Xe measurements. A current best estimate for Δ would be 1.07×10^8 yr for a "smooth" slowly varying, time-dependent model of nucleosynthesis and 1.56×10^8 yr for nucleosynthesis models with a large "spiked" event just before solar system separation. These values of Δ were calculated using the techniques of Schramm and Wasserburg¹¹ with the parameters shown in Table 1. Although the parameter values in Table 1 differ slightly from those used in ref. 11, the range of uncertainty estimated¹¹ is still valid (though possibly overcautious). Therefore the possible range of values for Δ of

$$7.5 \times 10^7 \text{ yr} \leq \Delta \leq 2.5 \times 10^8 \text{ yr} \quad (1)$$

given by Schramm and Wasserburg will be used here. This range includes the maximum effect of large time-dependent variations in the production function as well as any uncertainties in parameter values. Carbonaceous chondrites are generally regarded as more primitive than other meteorites (compare ref. 13). Their I-Xe ages seem, however, to be within $\sim 10^6$ yr of other meteorites^{14,15}. Thus the limits on Δ given above should apply to the carbonaceous chondrites.

The implied abundance ratio of element SH relative to ²⁴⁴Pu can be calculated from the values given in Table 1, *B* and *C*. For Type I carbonaceous chondrites the ratio is

$$\text{SH}/^{244}\text{Pu} \approx 0.0012/F \quad (2)$$

where *F* is the ¹³⁶Xe fission yield for element SH. The branching ratio of alpha decay to spontaneous fission decay for SH is unimportant because all alpha decays will eventually lead to a spontaneously fissioning nucleus (see ref. 1). There may be an uncertainty of a factor of 2 at most in the constant 0.0012. That uncertainty is, however, small compared with the uncertainty in *F*. At present it is not known whether SH will fission symmetrically, asymmetrically or even tertiarily, nor is the precise size of the fission yield known.

Asymmetric fission was shown by Schramm and Fowler to enable a fit to the rare Earth *r*-process abundance hump. To fit the CCF Xe spectrum with asymmetric fission it would be

necessary for the low mass asymmetric peak to be at $A \geq 135$ and, if the high mass peak is at $A \sim 155$, this would imply a lower average neutron multiplicity than predicted by Nix¹⁶. For asymmetric fission the peak yield can be estimated from systematics (see Hyde¹⁷) as $\sim 6\%$. Recently John *et al.*¹⁸ found that ^{257}Fm spontaneously fissions with a small symmetric component. If this effect is extrapolated to the superheavy region (a somewhat risky procedure) it tends to imply that element SH fissions symmetrically. The symmetric peak would be at $A \sim 142$, depending on the average neutron multiplicity and the number of alpha decays preceding the fissioning of SH. For symmetric fission the ^{136}Xe yield might be as high as 10% . The yield might, however, also be low ($\sim 1\%$) if the symmetric peak was far above Xe. Nix¹⁶ has also pointed out the possibility that tertiary fission may be the dominant mode for superheavy nuclei. If this is true it would be necessary for Xe to lie on the low mass side of one of the mass peaks in order to fit the CCF spectrum. In view of the uncertainties, calculations have been carried out for

$$0.01 \leq F \leq 0.10 \quad (3)$$

allowing a full order of magnitude uncertainty in F .

The implied half life, τ_{SH} , for the superheavy element can be calculated from the equation used by Schramm and Fowler¹. For a production function with a terminal "spike"

$$\tau_{\text{SH}} = \frac{\tau_{244}}{1 - \left[\frac{\tau_{244} \ln R(244, \text{SH})}{0.693 \Delta} \right]} \quad (4a)$$

and with a "smooth" production function

$$\tau_{\text{SH}} = \frac{\tau_{244}}{1 - \frac{\tau_{244}}{0.693 \Delta} \left\{ \ln [R(244, \text{SH})] + \ln \left(\frac{\tau_{244}}{\tau_{\text{SH}}} \right) \right\}} \quad (4b)$$

where (using the notation of ref 11)

$$R(244, \text{SH}) = \left(\frac{\text{SH}}{^{244}\text{Pu}} \right) \cdot \left(\frac{P_{244}}{P_{\text{SH}}} \right)$$

It was found that τ_{SH} implied from CCF is always much less than the duration of r -process nucleosynthesis, T_G ($T_G >$ the mean age of the elements $\sim 3 \times 10^9$ yr, see ref. 11) and terms varying as $\exp[-\ln(2) T_G / \tau_{\text{SH}}]$ have been neglected in equation (4). (This assumption might not be valid for $Z_{\text{SH}} \leq 114$, because then P_{SH}/P_{244} can become small, necessitating a large τ_{SH} to fit CCF data. Experimental searches^{19,20} for superheavy elements, however, indicate $\tau_{\text{SH}} < 10^9$ yr.)

Table 1 Input Data (Notation defined in Ref. 11)

	Ref-
(A) r -process production ratios	
$P_{129}/P_{127} \approx 1.52$	12
$P_{244}/P_{232} \approx 0.49$	11
$0.75 \geq P_{\text{SH}}/P_{244} \geq 0.20^*$	1
(B) Observed abundance ratios (4.6×10^9 yr ago)	
$^{129}\text{I}/^{127}\text{I} = 1.07 \times 10^{-4}$	24
$^{232}\text{Th}/^{238}\text{U} \approx 2.42$ (= 3.9 now)	11
$^{244}\text{Pu}/^{232}\text{Th} \approx 0.0054$ ($^{244}\text{Pu}/^{238}\text{U} = 0.013$)	25
(C) Mean carbonaceous chondrite abundances	
$\langle ^{238}\text{U} \rangle \sim 17$ p.p.b. (now) ~ 34 p.p.b. (4.6×10^9 yr ago)	26,27,28
$\langle ^{136}\text{Xe} \rangle$ CC $\approx 5 \times 10^{-11}$ CC STP/g	29
$\langle ^{136}\text{Xe} \rangle$ CC $\approx 4 \times 10^{-11}$ CC STP/g	

* This lower bound assumes $Z_{\text{SH}} = 110$; if $Z_{\text{SH}} = 114$, the lower bound goes to ~ 0.006 .

Values for P_{SH}/P_{244} are given in Table 1 and values for $\text{SH}/^{244}\text{Pu}$ are given by equation (2) using relation (3). Thus the range taken for $R(244, \text{SH})$ is

$$0.016 \leq R(244, \text{SH}) \leq 0.60 \quad (5)$$

A best estimate for $R(244, \text{SH})$ would probably be ~ 0.032 . Using the values for Δ given previously, this yields a best estimate of $\tau_{\text{SH}} \sim 2.3 \times 10^7$ yr for a spiked model and $\tau_{\text{SH}} \sim 2.4$

$\times 10^7$ yr for a smooth production model. If the entire range of values given by relations (1) and (5) are considered, the following limits can be placed on τ_{SH} implied from CCF Xe

$$1.65 \times 10^7 \text{ yr} \leq \tau_{\text{SH}} \leq 6.80 \times 10^7 \text{ yr} \quad (6)$$

This narrow range seems consistent with the implied $\tau_{\text{SH}} \sim 3.5 \times 10^7$ yr calculated in ref. 1 from the fossil track experiment of Bhandari *et al.*²¹. Carver and Anders²², however, have mentioned that a chemical fractionation factor of $\sim 10^4$ should be used in estimating the implied $\text{SH}/^{244}\text{Pu}$ ratio for the solar system from the results of ref. 21. Such a factor changes the half life implied from fossil tracks to $\sim 1.6 \times 10^8$ yr. The range of uncertainty of this number is, however, large enough to intersect the range given in relation (6). It is also important to note that Bhandari *et al.*²¹ found a high superheavy element track density in the meteorite Angra dos Reis and yet Hohenberg²³ found the fission Xe spectrum to be that of ^{244}Pu not CCF. This implies that the tracks observed by Bhandari *et al.*²¹ do not come from the same source as CCF, thus equality between the implied half lives would be purely coincidental. It should be mentioned that the validity of the experiment of Bhandari *et al.* is in serious question on several points (E. A. Carver and F. Anders, to be published; and P. B. Price, R. S. Rajan and E. K. Shirk, private communication) which will not be discussed here.

The half life implied in relation (6) for a superheavy element producing CCF Xe is long enough to ensure that there would be sufficient SH remaining at the time of track retention ($\sim 3.8 \times 10^8$ yr after Xe retention⁴) to produce a countable fossil fission track density. It is therefore important for observers to look at the fossil tracks in carbonaceous chondrites. Unfortunately, this is very difficult to do because of the small grain size in this meteorite class.

The half life implied from CCF Xe is within an order of magnitude of the Nilsson *et al.*²³ estimate for $^{294}110$ but it is many orders of magnitude longer than Nilsson half life estimates for other superheavy elements produced in the r -process. In particular elements $Z = 112$ to 119 correlated by Anders and Heymann⁷ with CCF Xe, are predicted by Nilsson to be relatively less stable than $^{294}110$. Although the estimates of Nilsson *et al.* may be uncertain by several orders of magnitude, estimates of relative stability should be fairly reliable.

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- Schramm, D. N., and Fowler, W. A., *Nature*, **231**, 103 (1971).
- Rowe, M. W., *Geochim. Cosmochim. Acta*, **32**, 1317 (1968).
- Alexander, E. C., Lewis, R. C., Reynolds, J. H., and Michel, M. C., *Science*, **172**, 837 (1971).
- Wasserburg, G. J., Huneke, J. C., and Burnett, D. S., *Phys. Rev. Lett.*, **22**, 1198 (1969).
- Wasserburg, G. J., Huneke, J. C., and Burnett, D. S., *J. Geophys. Res.*, **74**, 4221 (1969).
- Hohenberg, C. M., Munk, M. N., and Reynolds, J. H., *J. Geophys. Res.*, **72**, 3139 (1967).
- Anders, E., and Heymann, D., *Science*, **164**, 821 (1969).
- Srinivasan, B., Alexander, E. C., Manuel, O. K., and Troutner, D. E., *Phys. Rev.*, **179**, 1166 (1969).
- Dakowski, M., *Earth Planet. Sci. Lett.*, **6**, 152 (1969).
- Rao, M. N., *Nucl. Phys.*, **A140**, 69 (1970).
- Schramm, D. N., and Wasserburg, G. J., *Astrophys. J.*, **162**, 57 (1970).
- Fowler, W. A., in *Cosmology, Fusion, and Other Matters: A Memorial to George Gamow* (edit. by Reines, F.) (Colorado Associated University Press, Boulder, in the press, 1971).
- Anders, E., *Space Sci. Rev.*, **3**, 583 (1964).
- Podosek, F. A., and Hohenberg, C. M., *Earth Planet. Sci. Lett.*, **8**, 443 (1970).
- Podosek, F. A., *Geochim. Cosmochim. Acta*, **34**, 341 (1970).
- Nix, J. R., *Phys. Lett.*, **30B**, 1 (1969).

- ¹⁷ Hyde, E. K., *Nuclear Properties of the Heavy Elements III* (Prentice-Hall, Englewood Cliffs, New Jersey, 1964).
- ¹⁸ John, W., Hulet, E. K., Loughheed, R. W., and Wesolowski, J. J., *Phys. Rev. Lett.*, **27**, 45 (1971).
- ¹⁹ Nilsson, S. G., Thompson, S. G., and Tsang, C. F., *Phys. Lett.*, **28B**, 458 (1969).
- ²⁰ Grimm, W., Hermann, G., and Schössler, H. D., *Phys. Rev. Lett.*, **26**, 1040 (1971).
- ²¹ Bhandari, N., Bhat, S., Lal, D., Rajagopalan, G., Tamhane, A. S., and Venkatavaradan, V. S., *Nature*, **230**, 219 (1971).
- ²² Hohenberg, C. M., *Geochim. Cosmochim. Acta*, **34**, 184 (1970).
- ²³ Nilsson, S. G., Tsang, C. F., Sobiczewski, A., Szymanski, Z., Wycech, S., Gustafson, C., Lamm, I. C., Möler, P., and Nilsson, B., *Nucl. Phys.*, **A131**, 1 (1969).
- ²⁴ Hohenberg, C. M., Podosek, F. A., and Reynolds, J. H., *Science*, **156**, 202 (1967).
- ²⁵ Podosek, F. A., *Earth Planet. Sci. Lett.*, **8**, 183 (1970).
- ²⁶ Morgan, J. W., and Lovering, J. F., *Nature*, **213**, 873 (1967).
- ²⁷ Lovering, J. F., and Morgan, J. W., *J. Geophys. Res.*, **69**, 1979 (1964).
- ²⁸ Reed, jun., G. W., Kigoshi, K., and Turkevich, A., *Geochim. Cosmochim. Acta*, **20**, 122 (1960).
- ²⁹ Mazon, E., Heymann, D., and Anders, E., *Geochim. Cosmochim. Acta*, **34**, 781 (1970).

Isotopic Composition of Xenon from a Greenland Anorthosite

FOLLOWING a study of the data resulting from the lunar α -scattering experiment of Turkevich *et al.*¹, Olsen² pointed out that the rocks in the Mare Tranquillitatus region were similar in oxide composition to certain terrestrial anorthosites. When lunar rocks became available for examination after the Apollo 11 mission, O'Hara³ drew attention to the possibility that terrestrial anorthosites might be re-worked slices of the Earth's primordial crust. Windley⁴, in particular, supported this view proposing that early Pre-Cambrian Group III anorthosites are fragments of layered differentiates formed early in the evolution of the Earth. Romey⁵, on the other hand, does not like the comparison between lunar and terrestrial anorthosites and suggests that theories of lunar petrogenesis are developing too fast.

Nevertheless Windley's Group III anorthosites do seem to be of extreme age ($\approx 3,500$ m.y.) and to represent samples from an early phase of the Earth's geological history. If positive evidence concerning the events which occurred during this period is to be found, then this class of rock may provide a source of useful information. With this approach in mind, and being keenly aware of some of the major isotopic problems present in noble gas studies, it seemed reasonable to obtain samples of these perhaps very special rocks and to study the isotopic compositions of the noble gases "trapped" within them.

Six samples of Group III anorthosites from the West Greenland Fiskenaesset region were obtained from Dr B. F. Windley of the Greenland Geological Survey. It was decided to examine only the xenon in these rocks in the first instance.

Early measurements demonstrated the presence in rock No. 86943 (Windley classification) of xenon with an isotopic composition similar to that found in North American gas wells by Butler *et al.*⁶ and Clarke and Thode⁷ and discussed by

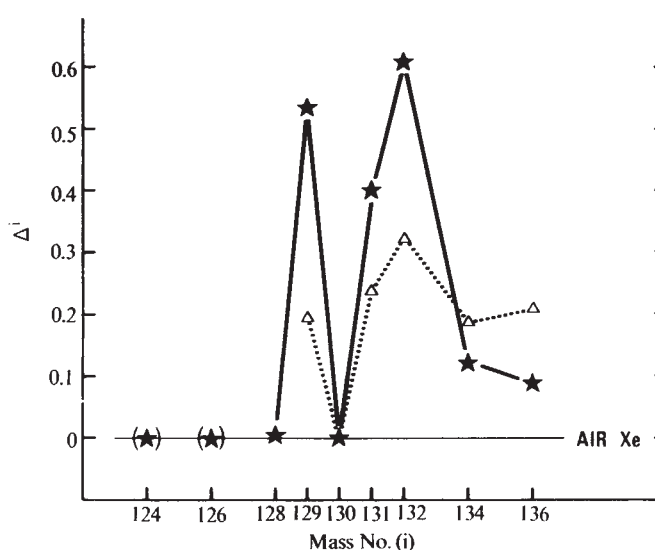


Fig. 1 Xenon ratio differences, $\Delta^1 = (\text{Xe}^i/\text{Xe}^{130})_{\text{sample}} - (\text{Xe}^i/\text{Xe}^{130})_{\text{air}}$, as a function of mass number (i) for 86943 anorthosite fines (> 700°C) and C-16 gas well xenon⁸. ★, 86943F (> 700°C); Δ, C-16.

Bennett and Manuel⁸. This, in itself, is an observation of some interest when considered in the light of the measurements of Srinivasan *et al.* (private communication) on a New South Wales iodyrite (International Mineralogical Association classification: iodyrite) who also report the presence of gas well type xenon. This xenon may be present on a world wide scale in suitable trapping sites.

Further studies on rock No. 86943 and its mineral concentrates have resulted in the observation of xenon with an isotopic composition which seems to be necessary if the gas well pattern is to be understood. Both Bennett and Manuel⁸ and Reynolds⁹ have pointed out that gas well xenon must contain a contribution from ²³⁸U spontaneous fission xenon, so that by subtracting this contribution from the gas well pattern it is possible to predict the isotopic composition of the remaining xenon. The isotopic composition of xenon observed in the fines of rock No. 86943 seems to approximate to that required for this component. In fact the presence of the gas well pattern in the total rock tends to support this view. The fines were produced as the result of the separation of the mineral concentrates from the total rock.

The xenon isotopic pattern observed in the fines (> 700°C) from rock No. 86943 is shown in Fig. 1 together with the gas well xenon pattern as typified by C 16 (ref. 8). Here the data are presented in terms of ratio differences from air xenon using ¹³⁰Xe as the normalizing isotope.

In Table 1 the isotopic ratios of the fines xenon, 86943F (> 700°C), are listed relative to ¹³⁰Xe. An estimate of the amount of anomalous ¹³²Xe in the fines, based on the assumption that all the ¹³⁰Xe is due to air xenon, is included. Blanks (< 10% of total xenon) and air standards were run with every sample. Xenon from a number of samples of total rock No.

Table 1 Isotopic Composition of 86943F (> 700°C) Xenon *

124	126	128	129	130	131	132	134	136
(0.022)	(0.021)	0.477 ± 0.020 †	7.024 ± 0.090	1.000	5.596 ± 0.085	7.210 ± 0.07	2.683 ± 0.035	2.266 ± 0.020

* Anomalous ¹³²Xe per gram of fines $\approx 2 \times 10^{-12}$ cc NTP—assuming all ¹³⁰Xe due to air.

† All errors are one standard deviation.

86943, its mineral concentrates and fines has been examined. The data reported here are the best obtained so far, although the ^{124}Xe and ^{126}Xe ratios are ill defined extrapolated values.

All my attempts to produce a model to synthesize the fines xenon isotopic pattern in terms of xenon components of proven isotopic composition have been unsatisfactory. A model of particular interest would be one based on linear mass fractionation of the type of xenon found in the $1,000^\circ\text{C}$ fraction of the meteorite Pesyanoe (ref. 10) but the low 134 and 136 abundances relative to the 132 mass abundance in the fines makes such a model untenable. Addition of ^{130}Xe to atmospheric xenon, but not to rock xenon, by a mechanism due to Cameron¹¹ is not an acceptable model for the same reason. No nuclear reaction has yet been proposed which can produce the anomalous pattern *in situ* assuming that the ^{129}Xe , ^{131}Xe and ^{132}Xe abundances are well correlated. The assumption of good correlation seems to be reasonable on the basis of the widespread observations of the gas well pattern. The low abundances of the ^{134}Xe and ^{136}Xe relative to ^{132}Xe in the fines xenon would be difficult to account for if a fission process was considered in order to explain the ^{129}Xe , ^{131}Xe and ^{132}Xe excesses.

On present evidence *in situ* production of the fines xenon component does not seem as likely as a model which accounts for it in terms of sampling, at least 3,210 m.y. ago (K/Ar age cited by Windley⁴), from a reservoir containing xenon with an isotopic composition similar to that of the fines xenon. The isotopic composition of such reservoir xenon could be the result of thorough mixing of more than one xenon component thus avoiding the difficulty associated with ascribing it to a single component. Sampling, at different periods throughout geological time, from this reservoir could account for all the gas well type xenon so far observed if suitable additions to the isotopic pattern are made by the inclusion of xenon from ^{238}U spontaneous fission.

The absence of any trace of the gas well pattern in xenon extracted from rocks of extreme age, or of supposedly deep seated origin, such as eclogites, ilmenite nodules and peridotites would then be considered to be due to their thorough outgassing during periods of metamorphism.

Thus, to summarize, first the West Greenland anorthosite No. 86943 contains gas well type xenon. Not all samples show this pattern, which is not surprising when the mineral heterogeneity of the bulk sample is considered. Second, the fines, produced during the concentration of the major minerals in the rock, contain a xenon component which seems to be essentially gas well type xenon with most of its ^{238}U spontaneous fission xenon component removed. Third, the origin of the fines xenon component is unknown. It is suggested that on the basis of present evidence it seems more likely to have originated from sampling of a suitable reservoir than to have been produced *in situ*.

The demonstrated presence of gas well type xenon in North America, Australia and Greenland indicates that it may be present on a global scale in suitable trapping sites. This observation of anomalous xenon in the fines of a Group III anorthosite substantiates previous suggestions concerning the origin of the gas well isotopic pattern. It does not, however, answer the vital question of how the anomalous xenon was produced. For example, have Group III anorthosites simply provided suitable trapping sites for the fines xenon component or has their possible presence in a primitive crust of the Earth some further significance in relation to the present day isotopic composition of gas well and atmospheric xenon?

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- ¹ Turkevich, A. L., Franzgrote, E. J., and Patterson, J. H., *Science*, **165**, 277 (1969).
- ² Olsen, E., *Science*, **166**, 401 (1969).
- ³ O'Hara, M. J., *Nature*, **225**, 321 (1970).
- ⁴ Windley, B. F., *Nature*, **226**, 333 (1970).
- ⁵ Romey, W. D., *Science*, **172**, 292 (1971).
- ⁶ Butler, W. A., Jeffery, P. M., Reynolds, J. H., and Wasserburg, G. J., *J. Geophys. Res.*, **68**, 3283 (1963).
- ⁷ Clarke, W. B., and Thode, H. G., in *Isotopic and Cosmic Chemistry* (edit. by Craig, H., Miller, S. L., and Wasserburg, G. J.), 471 (North-Holland, Amsterdam, 1964).
- ⁸ Bennett, G. A., and Manuel, O. K., *Geochim. Cosmochim. Acta*, **34**, 593 (1970).
- ⁹ Reynolds, J. H., *Proc. Intern. Conf. Mass Spec. Kyoto*, 594 (1970).
- ¹⁰ Marti, K., *Science*, **165**, 1263 (1969).
- ¹¹ Cameron, A. G. W., *Icarus*, **1**, 13 (1962).

Carbon-13 NMR Study of the Stereochemistry of Steranes from Oil Shale of the Green River Formation (Eocene)

THE presence of steranes and triterpanes in oils and sediments is especially significant as they possess the appropriate chemical stability for preservation throughout terrestrial diagenesis. They also contain a high degree of molecular order in their carbon skeleton and so the probability of their being synthesized by abiological processes is minimized. The high level of latent information in the structure and stereochemistry of these compounds renders them ideal biological markers¹⁻⁶.

The determination of true steric configuration is important in the study of both linear and cyclic fossil isoprenoids. For example, the structural information gathered on these compounds could be used in an analogous manner to the chemotaxonomic conclusions of recent flora. The biosynthesis of steroids and triterpenoids has now been shown to follow the biogenetic isoprene rule by squalene epoxide⁷⁻¹¹. In steroid and triterpenoid biosynthesis it has been established that the role of the enzyme is critical in specifying the unique chain folding which allows the annelation steps to proceed along one path—a path not favoured thermodynamically^{12,15}. The study of the stereochemistry of fossil steroids and isoprenoids is expected to lead to the understanding of the evolution of enzyme systems governing biosynthesis of their precursors as well as to the unravelling of the nature and kinetics of diagenetic processes in carbonaceous sediments.

The occurrence and the significance of steranes and triterpanes in the bitumen of Green River Shale has been well documented¹⁶⁻²². Gross identification of the individual biogenic compounds presented in these works was achieved on the basis of combined data from gas chromatography and mass spectrometry or X-ray crystallography²³⁻²⁵.

In our study milligram quantities of high purity C_{27} , C_{28} steranes were isolated from Green River Shale using a preparative gas-liquid chromatography system developed in this laboratory for milligram-scale automatic separation of individual components from complex mixtures (see refs. 26 and 27 for details). The ^{13}C nuclear magnetic resonance (NMR) spectra of the isolated steranes were determined on a Varian HR-60 NMR spectrometer, modified for high resolution pulsed Fourier transform spectroscopy. Equipment and operational details have been described elsewhere^{28,29}. Proton decoupling was performed both in the noise mode (simultaneously decoupling all protons) or off-resonance in the coherent mode to assist in spectral identification. The sample probe accommodated 5 mm sample tubes, and contained separate 15.08 MHz ^{13}C r.f. pulse coils and 60 MHz ^1H (proton) decoupling coils. For some spectra, the receiver coil was double tuned to allow internal locking on deuterium

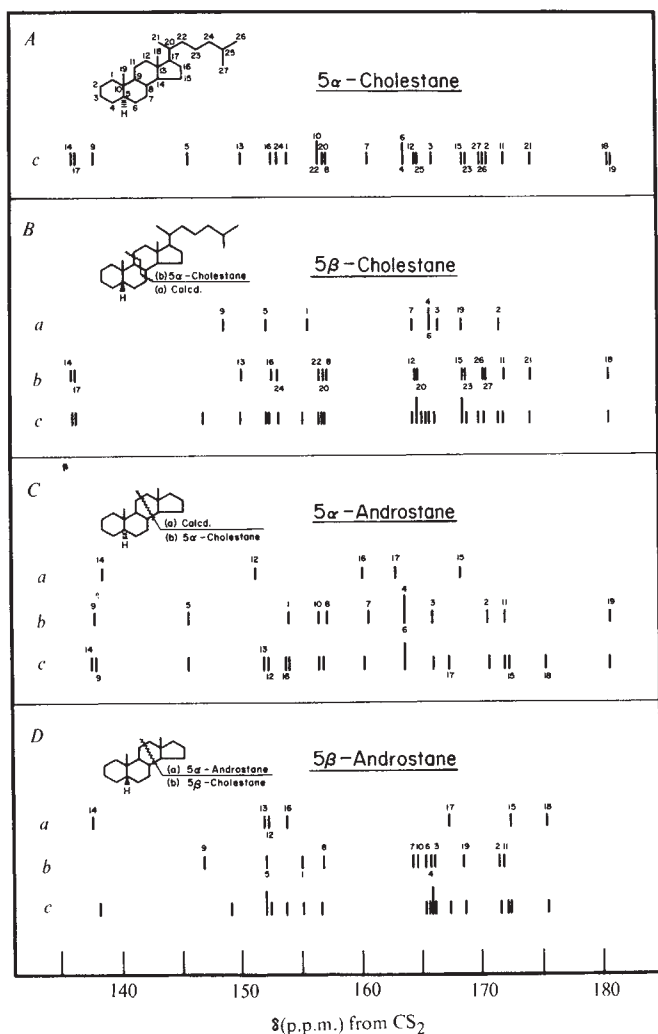


Fig. 1 Line diagram of the proton-decoupled ^{13}C resonances. *a*, Lines: resonance values calculated by additive parameters. *b*, Lines: chemical shifts taken from the appropriate part of the reference compound. *c*, Lines: experimental values. *A*, 5α-Cholestane 29 mg (Sigma Chemical Company). *B*, 5β-Cholestane 30 mg (Sigma Chemical Company). *C*, 5α-Androstane 30 mg (Mann Research Laboratory). *D*, 5β-Androstane 10 mg (Mann Research Laboratory).

(9.211 MHz). For the rest of the spectra, the ^{13}C transmitter coil was double tuned to allow coherent decoupling at the solvent deuterium frequency for a sharp reference peak, and an external ^7Li lock (23.321 MHz) was used which gave a limiting ^{13}C spectral resolution of approximately 0.03 p.p.m. CDCl_3 was used as the solvent in all cases. Due to the small quantity of the collected terpanes available (5–15 mg), both 30 μl . spherical microbulbs and normal 5 mm NMR tubes were used for spectra. The microbulbs were found to give increased sensitivity at the expense of resolution. For each spectrum, free-induction decays following 400 W r.f. pulses were accumulated on a Fabri-Tek model 1074 signal averager with a 4096 word memory. The accumulated signal was then Fourier transformed into a magnitude spectrum electronically using a Spectrum Analyser, or into an absorption spectrum numerically by a CDC 6600 computer.

The spectra were evaluated on the basis of the ^{13}C NMR behaviour of authentic tetracyclic steranes, model compounds and published data. The chemical shift assignments were aided by the extensive use of the qualitative rule of structural additivity derived for perhydrophenanthrenes³⁰, steroids, terpanes and carotenes^{31,32}.

Line diagrams of the noise decoupled ^{13}C NMR spectra of

the stereoisomers cholestane (5α-cholestane) and coprostanol (5β-cholestane) are shown in Fig. 1 (*A* and *B*, *c* lines). Both of these compounds have been tentatively identified in the Green River shale²¹. The difference between the two isomers lies in the manner of fusion of the A and B rings whereby the bridgehead carbons are *trans* or *cis*-substituted. To aid structural assignment, two additional model compounds, 5α-androstane and 5β-androstane, were selected and served as additive basic structural building units (*C* and *D*).

Our identification of ^{13}C resonances of cholestane followed the assignment reported by Rich *et al.*³¹. These assignments formed the basis of our extended calculations, where a part of the structure of cholestane was utilized (*b* lines). The calculations of the ^{13}C chemical shifts for the remaining portion of the molecule were performed by using the additive methyl substituent parameters obtained by factor analysis of methyl cyclohexanes by Dalling (ref. 30; *a* lines).

Although the spectrum of coprostanol (5β-cholestane) seems different from cholestane, closer examination reveals that the effect of the A ring inversion on the chemical shifts can easily be traced to simple gauche-anti spatial orientation changes of the C-2, C-3 and C-4 carbons of the A ring relative to γ carbons in the B ring. As expected, the C_{19} methyl has been deshielded (–) and its resonance shifted downfield by about 12 p.p.m. The C-9 and C-7 carbons were affected the strongest by the C-2 and C-4 carbons respectively which folded into a close gauche position causing 8 and 5 p.p.m. shifts upfield. The resonances of the C-2, C-3 and C-4 carbons moved slightly upfield due to the sum of deshielding (–) and shielding (+) effects of the neighbouring carbons. The bridgehead carbons of C-5 and C-10 could be assigned with a high degree of confidence on the basis of their carbon-proton coupling behaviour shown in the coherent decoupled spectra.

The designation of the carbons of the five membered ring of the androstanes presented a particular problem because no parametric values are available to calculate ^{13}C resonances in perhydroindanes. Recourse was made to another set of parameters called “hydrocarbon parameters” developed by Dalling³⁰ to approximate the chemical shifts of those particular carbons. The values obtained are shown in Fig. 1, *C/a* lines.

Table 1 ^{13}C Chemical Shifts* and Assignments for the ^{13}C Spectra of Steranes

Carbon	5α-Androstane	5β-Androstane	5α-Cholestane†	5β-Cholestane	5α-Ergostane
1	154.9	154.9	154.0	155.1	154.0
2	170.5	171.3	170.5	171.4	172.2
3	165.9	165.7	165.9	166.1	166.1
4	163.6	165.6	163.6	165.6	163.6
5	145.6	152.2	145.7	152.1	145.6
6	163.6	165.6	163.6	165.4	163.6
7	160.2	165.1	160.6	164.3	160.5
8	156.8	156.4	157.2	156.9	157.2
9	138.0	148.9	137.9	146.9	137.9
10	156.4	165.3	156.5	164.7	156.4
11	171.8	171.8	171.9	171.8	173.9
12	152.2	151.8	164.5	164.7	164.5
13	151.9	151.8	150.1	150.0	150.2
14	137.6	138.0	136.1	136.2	136.2
15	172.2	172.1	168.5	168.4	168.4
16	153.7	153.5	152.6	152.3	152.5
17	167.2	167.1	136.4	136.4	136.5
18	175.2	174.2	180.6	180.6	180.2
19	180.5	168.4	180.7	168.4	180.4
20			156.9	156.7	156.4
21			174.1	174.0	178.3
22			156.1	156.5	158.9
23			168.8	168.8	161.9
24			153.1	153.1	153.5
25			164.7	165.1	175.1
26			170.2	169.8	161.3
27			169.9	170.2	170.6
28					171.9

* In p.p.m. upfield from CS_2 .

† Slightly different values have been reported by Roberts *et al.*³¹.

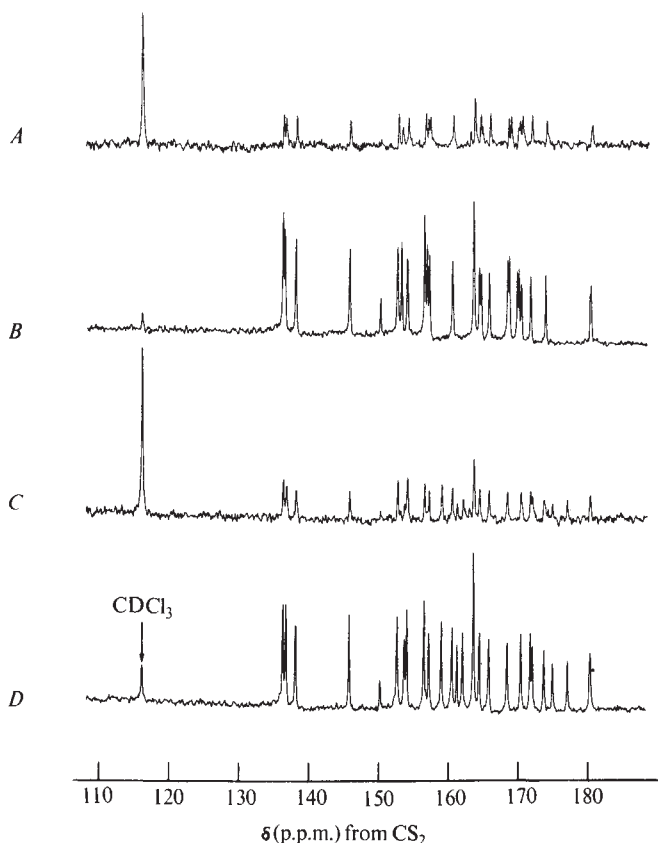


Fig. 2 Noise decoupled spectra of steranes. A, 12 mg sample of C_{27} sterane from Green River Shale. B, Authentic 5α -cholestane (Sigma). C, 8 mg sample of C_{28} sterane from Green River Shale. D, 5α -ergostane prepared from ergosterol (R & K Labs Inc.). Samples dissolved in 0.3–0.5 ml. $CDCl_3$ and placed in 5 mm sample tubes. Spectral conditions: 1H noise decoupled, solvent deuterium decoupled, 7Li external lock, ^{13}C r.f. pulse flip angle 50° , pulse interval 1.0 s, sample temperature $23^\circ C$, 5 kHz data sampling rate after r.f. pulse. 15,000–140,000 accumulated free-induction decays numerically fourier transformed.

Carbon resonances belonging to C-18, C-13 and C-14 positions were uniquely identified with the assistance of their proton coupling showing a quartet, a singlet and a doublet respectively.

The designation of carbon shifts of 5β -androstane provides a fine demonstration of the validity of the structural additivity principle. All assignments were derived using only the appropriate parts of 5α -androstane and 5β -cholestane molecules. ^{13}C NMR spectra possess fingerprint qualities corresponding to the multitude of factors affecting shifts, intensity and fine structure of the NMR signal. Both substituent³³ and conformational³⁴ effects have been shown to be very important. This quality of the ^{13}C NMR signal was demonstrated on the C_{27} and C_{28} steranes isolated from the hydrocarbon fraction of the oil shale bitumen of the Green River Formation. Their ^{13}C NMR noise decoupled spectra are present in Fig. 2 with the spectra of authentic 5α -cholestane and 5α -ergostane. There is no doubt that the spectra of the unknown steranes are identical with the spectra of the appropriate reference compounds.

On the basis of this spectral identity and study of model steranes we can state that the mode of ring fusions (all *trans*) and the geometric configuration of the side chain ($17\alpha[H]$) in the isolated C_{27} and C_{28} steranes follow unequivocally from the ^{13}C NMR data. The assignment of carbon resonances of 5α -ergostane was accomplished by using the assignments of 5α -cholestane for the cyclic portion of the molecule and aliphatic additive parameters³⁵ to calculate the side-chain shifts. The experimental ^{13}C chemical shift values of the investigated steranes are compiled in Table 1.

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- ¹ Bendoraitis, J. G., Brown, G. L., and Hepner, L. S., *Anal. Chem.*, **34**, 49 (1962); Bendoraitis, J. G., Brown, G. L., and Hepner, L. S., *Sixth World Petrol. Cong.*, Frankfurt (June, 1963).
- ² Eglinton, G., Scott, P. M., Belsky, T., Burlingame, A. L., Richter, W. J., and Calvin, M., *Adv. Org. Geochem.*, **41** (Pergamon Press, London, 1964).
- ³ Gelpi, E., Han, J., Nooner, D. W., and Oro, J., *Geochim. Cosmochim. Acta*, **34**, 965 (1970).
- ⁴ Johns, R. B., Belsky, T., McCarthy, E. D., Burlingame, A. L., Haug, P., Schnoes, H. K., Richter, W. J., and Calvin, M., *Geochim. Cosmochim. Acta*, **30**, 1191 (1966).
- ⁵ McCarthy, E. D., and Calvin, M., *Nature*, **216**, 642 (1967).
- ⁶ Studier, H. M., Hayatsu, R., and Anders, E., *Geochim. Cosmochim. Acta*, **34**, 965 (1970).
- ⁷ Eschenmoser, A., Ruzicka, L., Jeger, O., and Arigoni, D., *Helv. Chim. Acta*, **38**, 1890 (1955).
- ⁸ Ruzicka, L., *Proc. Chem. Soc.*, 341 (1959).
- ⁹ Clayton, R. B., *Quart. Rev.*, **19**, 168 (1965).
- ¹⁰ Van Tamelen, E. E., Willet, J. D., Clayton, R. B., and Lord, K. E., *J. Amer. Chem. Soc.*, **88**, 4752 (1966).
- ¹¹ Johnson, W. S., *Accounts of Chem. Research*, **1**, 1 (1968).
- ¹² Van Tamelen, E. E., Sharpless, K. B., Willet, J. D., Clayton, R. B., and Burlingame, A. L., *J. Amer. Chem. Soc.*, **89**, 3920 (1968).
- ¹³ Van Tamelen, E. E., Sharpless, K. B., Hanzlik, R., Clayton, R. B., Burlingame, A. L., and Wszolek, P. C., *J. Amer. Chem. Soc.*, **89**, 7150 (1967).
- ¹⁴ Van Tamelen, E. E., Hanzlik, R. P., Sharpless, K. B., Clayton, R. B., Richter, W. J., and Burlingame, A. L., *J. Amer. Chem. Soc.*, **90**, 3283 (1969).
- ¹⁵ Van Tamelen, E. E., Hanzlik, R. P., Clayton, R. B., and Burlingame, A. L., *J. Amer. Chem. Soc.*, **92**, 2137 (1970).
- ¹⁶ Burlingame, A. L., Haug, P., Belsky, T., and Calvin, M., *Proc. US. Nat. Acad. Sci.*, **54**, 1406 (1965).
- ¹⁷ Murphy, M. T. J., McCormick, A., and Eglinton, G., *Science*, **157**, 1040 (1967).
- ¹⁸ Hills, I. R., Whitehead, E. V., Anders, D. E., Cummins, J. J., and Robinson, W. E., *Chem. Comm.*, 752 (1966).
- ¹⁹ Henderson, W., Wollrab, V., and Eglinton, G., *Chem. Comm.*, 710 (1968).
- ²⁰ Henderson, W., Wollrab, V., and Eglinton, G., *Advances in Organic Geochemistry* (edit. by Schenk, P. A., and Havenaar, I.), **181** (Pergamon Press, 1968).
- ²¹ Gallegos, E. J., *Div. Petrol. Chem.*, **15**, (Chicago, September 1970).
- ²² Anders, D. E., and Robinson, W. E., *Geochim. Cosmochim. Acta*, **35**, 661 (1971).
- ²³ Hills, I. R., Smith, G. W., and Whitehead, E. V., *Div. Petrol. Chem.*, **13**, B5 (Atlantic City, New Jersey, September, 1968).
- ²⁴ Burlingame, A. L., and Schnoes, H. K., in *Mass Spectrometry in Organic Geochemistry* (edit. by Eglinton, G., and Murphy, M. T. J.), **89** (Springer, New Jersey, 1969).
- ²⁵ Schnoes, H. K., and Burlingame, A. L., in *Topics in Organic Mass Spectrometry* (edit. by Burlingame, A. L.), **369** (Wiley-Interscience, New York, 1970).
- ²⁶ Gelpi, E., Wszolek, P. C., Yang, E., and Burlingame, A. L., *J. Chrom. Sci.*, **9**, 147 (1971).
- ²⁷ Gelpi, E., Wszolek, P. C., Yang, E., and Burlingame, A. L., *Anal. Chem.*, **43**, 864 (1971).
- ²⁸ Jones, D. E., thesis, Univ. California, Berkeley (1970).
- ²⁹ Wilson, D. M., thesis, Univ. California, Berkeley (1971).
- ³⁰ Dalling, D. K., thesis, Univ. Utah, Salt Lake City (1969).
- ³¹ Reich, H. J., Jautelat, M., Messe, M. T., Weigert, F. J., and Roberts, J. D., *J. Amer. Chem. Soc.*, **91**, 7445 (1969).
- ³² Jautelat, M., Grutzner, J. B., and Roberts, J. D., *Proc. US Nat. Acad. Sci.*, **65**, 288 (1970).
- ³³ Cheney, B. V., and Grant, D. M., *J. Amer. Chem. Soc.*, **89**, 5319 (1967).
- ³⁴ Grant, D. M., and Cheney, B. V., *J. Amer. Chem. Soc.*, **89**, 5315 (1967).
- ³⁵ Grant, D. M., and Paul, E. G., *J. Amer. Chem. Soc.*, **86**, 2984 (1964).

BIOLOGICAL SCIENCES

Cellular Lipid Inclusions in the White Matter in Multiple Sclerosis

WE have obtained evidence to support the suggestion that fatty acid¹⁻⁴ and glycolipid⁴⁻⁸ metabolism is defective in the white matter outside acute plaques of multiple sclerosis. Histochemical techniques show two types of intracellular lipid inclusions in such apparently normal areas.

Seventeen plaques from eight cases of multiple sclerosis and white matter from sixteen control patients, who had died from other neurological diseases, were fixed in routine saline-formol and transferred to 1% calcium acetate—10% formalin. Frozen sections were stained with haematoxylin-eosin, Sudan black, Nile blue sulphate⁹, osmium tetroxide- α naphthylamine⁹, 'oil red O', modified periodic acid-Schiff⁹, gold-hydroxamic acid, acid haematein⁹, perchloric acid-naphthoquinone⁹, Holczinger's copper-rubeanic acid⁹, diazotization-coupling¹⁰, p-dimethylaminobenzaldehyde-nitrite¹¹, oxidized tannin-azo¹², trypan blue¹³, Hale's dialysed iron¹⁴, toluidine blue, Feulgen¹⁵ and methyl green-pyronin¹⁵. Sections were also stained with 'Luxol'-fast blue either with the standard technique¹⁵ or with the dye dissolved in 60% ethanol. Some tissue sections were extracted with acetone (4° C), chloroform-methanol (22° C; 2:1 v/v), or ethanol (22° C) for 1 h.

Plaques of multiple sclerosis were designated as: acute (six from two cases), showing perivenular infiltration with lymphocytes and plasma cells as well as hypercellularity throughout the lesion; active established chronic (five from five cases), with a rim of proliferating neuroglia at the edge; or inactive established chronic (six from four cases), with no evidence of cellular activity.

Two types of lipid inclusions were found in the white matter at least 2 cm away from acute plaques. The first consisted of granular or globular, isotropic droplets usually near neuroglial nuclei. The cells involved usually appeared to be oligodendroglia rather than astrocytes. These droplets were always stained with 'Luxol'-fast blue in 60% ethanol (Fig. 1) and, less frequently, with copper-rubeanic acid (Fig. 2), but not by the other methods used. They remained colloidal-iron negative after extraction with ethanol (see below). Extraction with chloroform-methanol extinguished staining with both Luxol-fast blue and copper rubeanic acid: acetone eliminated only

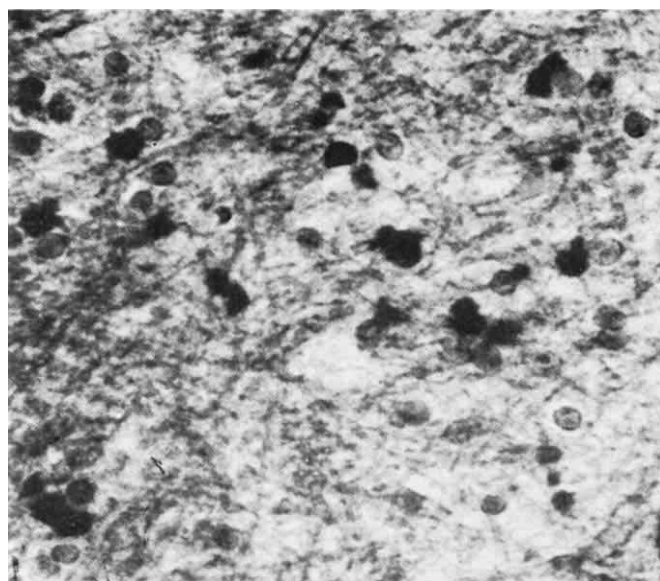


Fig. 2 As for Fig. 1 but stained by Holczinger's copper-rubeanic acid method for free fatty acids ($\times 960$).

the latter staining. The precise nature of the lipid in this type of inclusion is uncertain; the fatty acid reaction could be due to hydrolysis of phospholipids.

The second lipid inclusion in the white matter around acute plaques was a homogeneous relatively large mass usually close to or surrounding a cell nucleus. The cell type involved could not be clearly identified. The lipid mass was stained metachromatically pale orange-brown with cresyl violet (Fig. 3) and thionin (indicating sulphatide), metachromatically red with toluidine blue, moderately with 'Luxol'-fast blue, perchloric acid-naphthoquinone (cholesterol) and Alcian blue, and weakly with the modified PAS method (indicating a little cerebroside). These reactions were prevented by preliminary extraction with chloroform-methanol, but only that with perchloric acid-naphthoquinone was extinguished by acetone. The colloidal iron reaction became positive after extraction with ethanol, but not with acetone or ethanol followed by chloroform-methanol. This lipid inclusion was unstained by the other lipid methods used, the protein techniques and the methods for nucleic acids. With the exception of 'Luxol' preparations, however, it could always be identified in stained sections by its characteristic crystalline anisotropism (Fig. 3). Thus, this second inclusion seems to contain sulphatide, cholesterol and a little cerebroside, but no detectable mucosubstances. The colloidal iron reaction after ethanol recalls the staining of myelin lipids after acetone extraction¹⁷; the reversibility of both phenomena by chloroform-methanol extraction suggests that an acidic lipid (much as sulphatide) had been unmasked by ethanol or acetone.

Both lipid inclusions were frequently seen around acute plaques, but only occasionally around active and inactive chronic lesions. The first inclusion was never seen in control brains. Inclusions resembling the second type were noted in three control brains but were only occasionally related to cell nuclei. The nature of such extracellular inclusions is obscure¹⁸⁻²⁰; possibly they are derived from intracellular lipid accumulations²¹. Neither type of lipid inclusion in acute multiple sclerosis resembled the Rosenthal fibre²²: their negative reaction for protein, DNA and RNA indicates that they were not viral inclusion bodies.

The lipid inclusions within neuroglial cells are consistent with reports of abnormal proportions of lipids in white matter in multiple sclerosis¹⁻⁸. In particular, the second lipid inclusion may be responsible for the raised sulphatide/cerebroside ratio in multiple sclerosis white matter⁸.

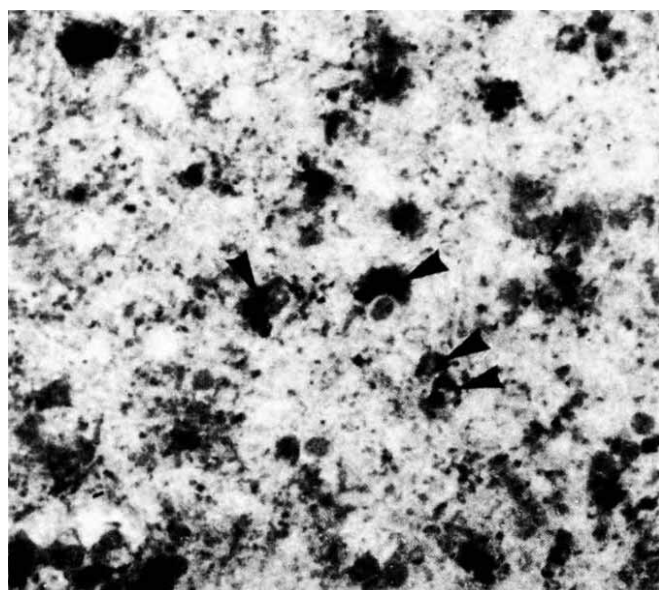


Fig. 1 Type 1 inclusion in cells resembling oligodendroglia (arrows) in the white matter around an acute plaque of multiple sclerosis. 'Luxol'-blue in 60% ethanol ($\times 960$).

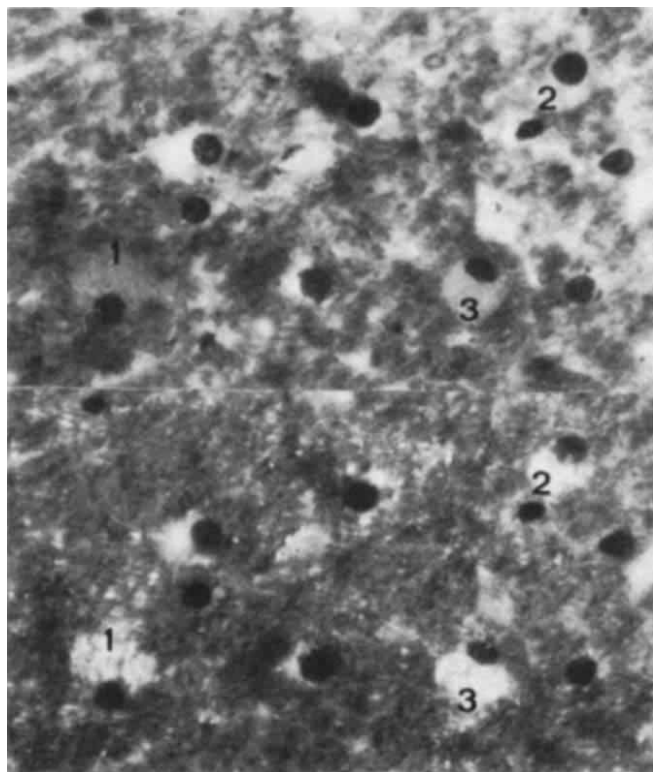


Fig. 3 Top: type 2 inclusion in the white matter around an acute plaque of multiple sclerosis. Note the intracellular site of these inclusions (1, 2, 3), which were stained metachromatically orange-brown in the section. Cresyl violet ($\times 960$). Bottom: same field viewed in polarized light.

The metabolic interpretation of the neuroglial lipid inclusions in multiple sclerosis is speculative. They could result from defective cerebroside synthesis and increased production of sulphatide or, possibly, by failure in extrusion of membrane from the myelin-formative cell, so that lipid accumulates intracellularly.

It is unlikely that the inclusions reflect remyelinating activity, because they occur in apparently normal white matter distant from the demyelinating edge and, moreover, their histochemical characteristics do not correspond with those of the lipids that temporarily appear within interfascicular oligodendroglia during myelination²³⁻²⁶.

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- ¹ Gerstl, B., Kahnke, M. J., Smith, J. K., Tavaststjerna, M. G., and Hayman, R. B., *Brain*, **84**, 310 (1961).
- ² Baker, R. W. R., Thompson, R. H. S., and Zilkha, K. J., *Lancet*, **i**, 26 (1963).
- ³ Stallberg-Stenhagen, S., and Svennerholm, L., *J. Lipid Res.*, **6**, 146 (1965).
- ⁴ Gerstl, B., Eng, L. F., Tavaststjerna, M., Smith, J. K., and Kruse, S. L., *J. Neurochem.*, **17**, 677 (1970).
- ⁵ Cumings, J. N., *Brain*, **76**, 551 (1953).
- ⁶ Davison, A. N., and Wajda, M., *J. Neurochem.*, **9**, 427 (1962).
- ⁷ Einstein, E. R., Dalal, K. B., and Csejtey, J., *J. Neurol. Sci.*, **11**, 109 (1970).
- ⁸ Cumings, J. N., and Goodwin, H., *Lancet*, **ii**, 664 (1968).
- ⁹ As set out in Adams, C. W. M., *Adv. Lipid Res.*, **7**, 1 (1969).
- ¹⁰ Glenner, G. G., and Lillie, R. D., *J. Histochem. Cytochem.*, **7**, 416 (1959).
- ¹¹ Adams, C. W. M., *J. Clin. Pathol.*, **10**, 56 (1957).

- ¹² Dixon, K. C., *Amer. J. Pathol.*, **35**, 199 (1959).
- ¹³ Adams, C. W. M., and Bayliss, O. B., *J. Histochem. Cytochem.*, **16**, 110 (1968).
- ¹⁴ Hale, C. W., *Nature*, **157**, 802 (1946).
- ¹⁵ As set out in Pearse, A. G. E., *Histochemistry: Theoretical and Applied*, third ed. (Churchill, London, 1968).
- ¹⁶ Heslinga, F. J. M., and Déierkauf, F. A., *J. Histochem. Cytochem.*, **10**, 704 (1962).
- ¹⁷ Adams, C. W. M., and Bayliss, O. B., *J. Histochem. Cytochem.*, **16**, 119 (1968).
- ¹⁸ Smith, M., *J. Neurol. Neurosurg. Psychiat.*, **12**, 100 (1949).
- ¹⁹ Wolman, M., *Confin. Neurol.*, **20**, 36 (1960).
- ²⁰ Ibrahim, M. Z. M., and Levine, S., *J. Neurol. Neurosurg. Psychiat.*, **30**, 545 (1967).
- ²¹ Ungar, H., Hine, C. H., Kodama, J. K., and Anderson, H. H., *Arch. Pathol.*, **60**, 139 (1955).
- ²² Herndon, R. M., Rubinstein, L. J., Freeman, J. M., and Mathieson, G., *J. Neuropathol. Exp. Neurol.*, **29**, 524 (1970).
- ²³ Adams, C. W. M., and Davison, A. N., in *Neurohistochemistry* (edit. by Adams, C. W. M.), 332 (Elsevier, Amsterdam, 1965).
- ²⁴ Adams, C. W. M., and Leibowitz, S., in *Structure and Function of Nervous Tissue* (edit. by Bourne, G. H.), **3**, 309 (Academic Press, New York, 1969).
- ²⁵ Mickel, H. S., and Gilles, F. H., *Brain*, **93**, 337 (1970).
- ²⁶ Adams, C. W. M., *Research on Multiple Sclerosis* (Thomas, Springfield, Illinois, in the press).

Nature of the Nerve Lesion caused by a Pneumatic Tourniquet

THE use of a tourniquet around a limb may be followed by damage to peripheral nerves. This lesion has been investigated experimentally in cats^{1,2} and the results suggested that in most cases the nerve damage was limited to the portion of the nerve under the tourniquet. Nerve fibres conducted normally distal to this level, with either a conduction block or a reduced conduction velocity at the site of the tourniquet. Histological studies showed localized demyelination under the tourniquet with preservation of axonal continuity. A similar pathological change is believed to occur in many forms of human pressure palsy, and the conclusion that the nerve damage is due to ischaemia has been widely accepted.

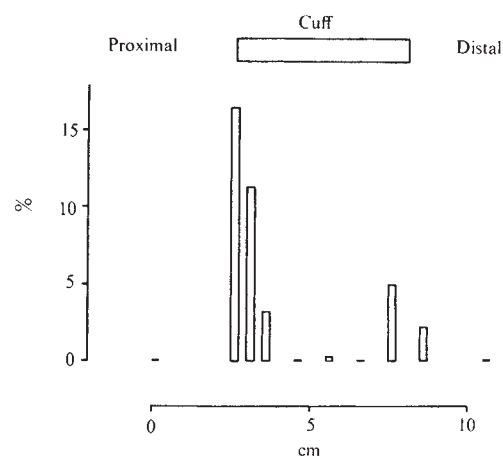


Fig. 1 Histogram to show proportion of abnormal fibres in transverse sections of medial popliteal nerve at different levels in relation to site of tourniquet (5 cm cuff inflated to 1,000 mm Hg for 75 min). Approximate position of cuff shown above. The specimen was taken 3 months after tourniquet. Nerve was fixed by perfusion with buffered glutaraldehyde and post-fixed with osmium tetroxide, 1 μ m transverse sections were stained with toluidine blue, photographed, and counts made from enlarged prints⁵. Abnormal fibres (axon diameter 5 μ m or greater, myelin thickness less than 1 μ m) are expressed as a percentage of the large fibre population.

We have recently done some further experiments using a sphygmomanometer cuff inflated to 1,000 mm Hg around the hind limb of the baboon for 1–3 h. A brief preliminary account of the technique and of the resulting changes in nerve conduction has been published³. In the course of the experiments, it became clear that the role of direct pressure in producing nerve damage had been underestimated by previous workers. Two pieces of evidence for this can be cited.



Fig. 2 Single fibres from medial popliteal nerve 1–4 days after tourniquet (5 cm cuff at 1,000 mm Hg for 90 min), to show different stages of invagination at nodes of Ranvier. Fibres were taken from a portion of nerve under the proximal edge of the cuff. Perfusion-fixation was carried out with buffered glutaraldehyde followed by post-fixation with osmium tetroxide; single fibres teased apart in 'Epon'. Symbol in *b* is as in Fig. 4.

(1) With the relatively wide (5 cm) cuff used, it has been found consistently that the nerve damage is maximal under the edges of the cuff or is restricted to these regions. This has been true of all stages of the pathological change in nerves studied for up to 6 months after the tourniquet had been applied. It has been demonstrated in single teased fibres and in transverse sections (Fig. 1). (2) The nature of the pathological change was best appreciated by examining single teased fibres by light and electron microscopy. In essence, the early change consisted of a dislocation of the nodes of Ranvier of the large-diameter myelinated fibres. The mildest form was a slight invagination of one paranode into the adjacent one (Figs. 2*a* and 3). Deeper invagination is illustrated in Figs. 2*b* and 4; it is accompanied by infolding of the myelin of the ensheathing paranode, with movement of the nodal axoplasm and axonal membrane away from the point of junction of the two Schwann cells. The dislocated nodal part of the axon could still be identified by the thickening of the axonal membrane and by the presence of characteristic transverse bands. The junction of adjacent Schwann cells was identified by their microvilli. If one assumes that the original site of the nodal axon was at the point of junction of the two Schwann cells, there must have been movement of the axon relative to the Schwann cell junction of approximately 30 μ m, accompanying the invagination of the paranodal myelin.

A penetration of one paranode by another deeper than that shown in Fig. 4 may occur. In longitudinal sections of single fibres, it has not been unusual to see invagination and move-



Fig. 3 Electron micrograph (LS) of nodal region shown in Fig. 2*a*.

ment of the nodal axon which has proceeded for 100–200 μ m. In such cases there is partial or complete rupture of myelin lamellae in the invaginated region.

All these changes are seen within the first few hours after the tourniquet and, by the end of the first week, invagination of myelin is beginning to be followed by demyelination. In our experience, the demyelination has usually been restricted to the region within 200 μ m of the node of Ranvier. Demyelination of whole internodal segments has been rare. When teasing single nerve fibres, the striking feature of these lesions is that the changes described above have a clear polarity, the direction of invagination always being away from the cuff towards uncompressed tissue.

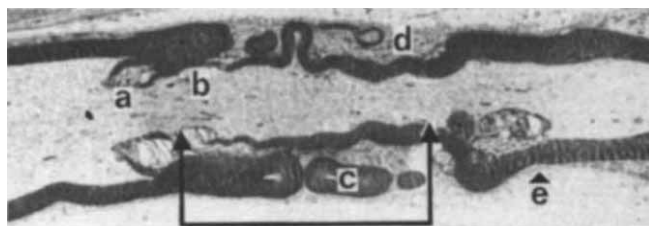


Fig. 4 Electron micrograph (LS) of nodal region shown in Fig. 2*b*. *a*, Terminal myelin loops of ensheathing paranode; *b*, terminal myelin loops of ensheathed paranode; *c*, myelin fold of ensheathing paranode cut tangentially; *d*, Schwann cell cytoplasm; *e*, microvilli indicating site of Schwann cell junction. Large arrows show length of ensheathed paranode (approximately 20 μ m).

From the nature of the lesion, its polarity, and its distribution under the cuff, we therefore conclude that it is due to direct pressure and not to ischaemia. The principal evidence cited by previous authors against the possibility of direct pressure producing nerve damage is the experiment described by Grundfest⁴, in which the pressure in an oxygenated chamber enclosing excised frog nerve was increased to approximately 1,000 atm before conduction was abolished. But this is a different situation from that which occurs when a pneumatic cuff is inflated around a limb. In the latter case there is a pressure gradient in the tissues between the portion under the cuff and that beyond its edge. Our results indicate that in these circumstances, longitudinal movement of the axon and its myelin occurs with respect to the Schwann cell. This change precedes demyelination.

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- ¹ Denny-Brown, D., and Brenner, G., *Arch. Neurol. Psychiat.*, **51**, 1 (1944).
- ² Mayer, R. F., and Denny-Brown, D., *Neurology*, **14**, 714 (1964).
- ³ Danta, G., Fowler, T. J., and Gilliatt, R. W., *J. Physiol.*, **215**, 50P (1971).
- ⁴ Grundfest, H., *Cold Spring Harbor Symp. Quant. Biol.*, **4**, 179 (1936).
- ⁵ Swallow, M., *J. Neurol. Neurosurg. Psychiat.*, **29**, 205 (1966).

Local Origin of Fibroblasts deduced from Sarcomas induced in Chimaeras by Implants of Pliable Disks

"THE origin of fibroblasts in healing wounds has been debated for over 100 years" according to Ross, Everett and Tyler¹, whose recent experiment indicated local connective tissue to be the source. Barnes and Khrushchov² had already concluded that in murine radiation-chimaeras precursor cells arose in bone marrow. We deduced from this that implants of pliable disks of adequate size, which give rise to local malignant growths (sarcoma), should in similar radiation-chimaeras produce sarcomas derived from the donor (myeloid) elements of the chimaeras and have examined such sarcomas induced by cellulose or silicone rubber. In the chimaeras the donated marrow was distinguishable from the host tissue by the presence in mitotic cells of *T6T6* chromosomes in one or other phase. The sarcomas were analysed cytologically; also, tumours arising in allogeneic chimaeras were tested genetically by their capacity for continued growth in mice of both pure strains. Both tests indicate that the tumours arose from the native host cells and not from the donated myeloid cells or their progeny.

The radiation-chimaeras used were of two kinds CBA/CBA-*T6T6* and CBA-*T6T6*/A produced as in a previous experiment³.

Five to six weeks after the establishment of chimaerism, the mice were anaesthetized with ether. An incision was made in the mid-line of the shaved skin of the back and into subfascial pockets on each side a disk was inserted, the wound being closed with Michel clips.

In one series the disks were of 'Millipore R' filter, 25 mm diameter and 0.10 μ m pore size. In the other series the disks were of silicone rubber (supplied by Dow Corning Center for Aid to Medical Research, Midland, Michigan)—2 cm in diameter and 1 mm thick⁴. Tumours once diagnosed were allowed to grow to an estimated diameter greater than 0.5 cm. Then the mouse was injected with 'Colcemid' and killed 4–5 h later. A portion of the tumour was macerated to produce cells suitable for chromosome analysis³. In a few mice the tumour presented not as a solid mass but as a fluctuant cyst around a rubber disk. At necropsy the cyst was aspirated of its bloodstained fluid which was then processed as a cell suspension. In each case a tumorous thickening of part of the cyst wall was identified.

Portions of solid tumour about 1 mm³ were passed by means of a Bashford needle (or cyst fluid by syringe and needle) to suitably compatible mice. Thus tumours in syngeneic chimaeras were passed to (CBA $\times$ CBA-*T6T6*) F₁ hybrids, where the single *T6* chromosome of dividing cells of the second host would distinguish cells of the second host from tumour cells, which should contain no *T6* chromosomes if the tumour arose from the natural cells of the primary host or two or more *T6* chromosomes if the tumour derived from the donated cells. Tumours in allogeneic chimaeras were analysed by passage to CBA, A and (CBA-*T6T6* $\times$ A) F₁ mice: tumours of host origin should grow in CBA and F₁ mice only, whereas those stemming from the donated bone marrow should grow in A and F₁ mice only. A single case of a subcutaneous sarcoma arising in a syngeneic chimaera of another experiment was analysed cytologically.

The fate of fifty-five chimaeras investigated is given in Table 1.

All tumours were analysed cytologically as of host origin (Table 2). In all but five a diagnosis was made on the primary tumour (or cyst fluid). In four of the five exceptions absence or paucity of mitoses in the primary tumour was the cause of doubt. Repetition on the first passage of the tumour confirmed that the primary tumour was of host origin. In the fifth animal (A390) a small tumour was diagnosed only at necropsy; passage was successful and the passaged tumour shown

(M6251) to have arisen from the host tissue of the original chimaera.

All twenty-one tumours arising in allogeneic chimaeras behaved in the genetic test of passage as host type tumours, killing CBA and (CBA-*T6T6* $\times$ A) F₁ recipients but not A strain mice.

Table 1 Fate of Radiation Chimaeras with Implants

Implant	Type of chimaera	Early death	Killed—tumour	Late death—no tumour
Cellulose	Syngeneic	4	7	0
	Allogeneic	0	13	2
Silicone rubber	Syngeneic	0	11	3
	Allogeneic	2	8*	5

*One died undiagnosed.

Table 2 Sarcomas in Subcutaneous Tissue

Type of chimaera	Code No.	Origin of tumour	Confirmation on passage Code No.	Origin
(a) Induced by 'Millipore' filter				
Allogeneic	M5595	—	M5625	Host
Allogeneic	M5616	Host		
Syngeneic	M5617	? Host	M6115	Host
Allogeneic	M5618	Host		
Syngeneic	M5631	Host		
Allogeneic	M5633	Host		
Allogeneic	M5634	Host		
Syngeneic	M5639	Host		
Syngeneic	M5640	Host		
Allogeneic	M5642	Host		
Allogeneic	M5648	Host		
Allogeneic	M5650	Host		
Allogeneic	M5653	Host		
Allogeneic	M5660	Host		
Syngeneic	M5664	Host		
Allogeneic	M5668	Host		
Allogeneic	M5675	Host		
Syngeneic	M5676	Host		
Syngeneic	M5679	Host		
Allogeneic	M5691	Host		
(b) Induced by silicone rubber				
Allogeneic	M5638	—	M5649	Host
Syngeneic	M5692	Host		
Syngeneic	M5695	Host		
Syngeneic	M5696	Host		
Syngeneic	M5702	Host		
Syngeneic	M5728	Host		
Allogeneic	M5733	Host		
Allogeneic	M5767	Host		
Allogeneic	M5768	Host		
Syngeneic	M5785	Host		
Syngeneic	M5786	—	M5864	Host
Syngeneic	M5791	Host		
Syngeneic	M5795	Host		
Syngeneic	M5796	Host		
Allogeneic	M5805	Host		
Allogeneic	M5825	Host		
Syngeneic	M5865	Host		
Allogeneic	M5915	—	(M6106) (M6153) M6251	Host Host Host
Allogeneic	A390	—		
(c) Spontaneous				
Syngeneic	M5579	? Host	M5623	Host

The one spontaneous sarcoma in a syngeneic chimaera was also typed cytologically as of the host.

The results indicate that sarcomas induced in the subcutaneous connective tissues of radiation chimaeras originate in cells of the host rather than in immigrant cells derived from the donated bone marrow. Similar results had been obtained for osteogenic connective tissue³: sarcomas induced in bone after injection of strontium-90 were also of host-type in spite of the immediate propinquity of donor-type bone marrow.

In this experiment none of forty tumours arose from the donated tissue. Long experience in this laboratory with chimerae induced after this lethal dose of radiation (1,000 rads) to CBA mice indicates that little, if any, reversion of myeloid or lymphoid tissues occurs, so that virtually all leucocytes were of donor origin⁵. The deduction to be made, therefore, is that these tumours, most of which were of spindle cells (fibrosarcoma), arose from native connective tissue cells, not immigrant leucocytes.

Ross *et al.*¹ came to a similar conclusion for the origin of normal fibroblasts in healing wounds of skin. This was deduced from experiments in which ³H-thymidine was given to one partner of an irradiated parabiotic pair of rats dependent for their marrow function and survival on the shielded femur of the animal that was later injected. Labelled leucocytes were found in skin wounds of both but labelled fibroblasts only in the animal exposed to ³H-thymidine.

The thesis of Barnes and Khrushchov² on which the present experiment was planned rested on the observation that, after 5–7 days' duration of a glass disk *in situ*, the predominant cell attached to the glass had the general microscopic appearance of a young fibroblast. Ryan and Spector⁶ apparently consider the cells on the glass to be a continuously maintained population of macrophages derived from blood monocytes, dividing after a latent interval of several days and departing. Spector⁷ concludes that macrophages, though capable of change to epithelioid cells in tissue, do not convert to fibroblasts.

The controversy that has lasted for more than a century on the origin of the fibroblast may still not be resolved and proponents of both options may be right. We deduce from the present study and that of Ross *et al.*¹ that malignant and normal fibroblasts can be derived from local precursors in connective tissue, but this does not exclude that in appropriate circumstances fibroblasts may be derived from myelogenous cells.

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- ¹ Ross, R., Everett, N. B., and Tyler, Ruth, *J. Cell Biol.*, **44**, 645 (1970).
- ² Barnes, D. W. H., and Khrushchov, N. G., *Nature*, **218**, 599 (1968).
- ³ Barnes, D. W. H., Carr, T. E. F., Evans, E. P., and Loutit, J. F., *Intern. J. Radiat. Biol.*, **18**, 531 (1970).
- ⁴ Brues, A. M., Auerbach, H., De Roche, Georgia M., and Grube, D. D., *Argonne National Laboratory, Biological and Medical Research Division, Annual Report, ANL-7535* (1968).
- ⁵ Micklem, H. S., and Loutit, J. F., *Tissue Grafting and Radiation*, Chapter 4 (Academic Press, New York, 1966).
- ⁶ Ryan, G. B., and Spector, W. G., *Proc. Roy. Soc., B*, **175**, 269 (1970).
- ⁷ Spector, W. G., *Series Haematologica* **3**, **2**, 132 (1970).

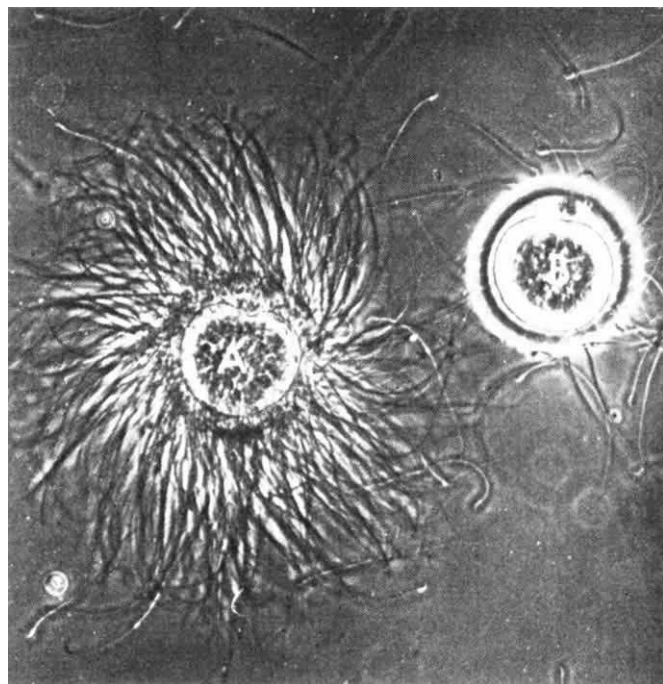


Fig. 1 Two eggs recovered from a hamster female 5 h after artificial insemination and incubated for 5 min with capacitated spermatozoa. One of the eggs (A) has many spermatozoa attached to the zona pellucida (negative zona reaction), while the other egg (B) does not have spermatozoa firmly attached to the zona (positive zona reaction) ($\times 450$).

cortical granule material which is released from the egg into the perivitelline space shortly after sperm entry into the egg cytoplasm. We wish to present experimental evidence to support this inference.

Adult female hamsters were induced to ovulate by injection of human chorionic gonadotropin on day 3 of the oestrous cycle³, counting the day of the post-oestrous discharge⁴ as day 1. Fourteen hours after the injection, they were artificially inseminated with cauda epididymal spermatozoa and killed at various times after insemination, from 5 to 72 h. Eggs were flushed from the oviducts with Tyrode solution and cumulus cells, if present, were removed by treatment with 0.1% bovine testicular hyaluronidase (300 USP units/mg, Nutritional Biochem. Corp.) in Tyrode solution. Cumulus-free eggs thus obtained were placed in 100 μ l. of an incubation medium⁵ under mineral oil in a sterile plastic dish. Spermatozoa used to inseminate these eggs were capacitated with detoxified human blood serum^{6,7}. Five minutes after the eggs and spermatozoa had been mixed together, two types of eggs could be distinguished (Fig. 1). One type had a cloud of actively moving spermatozoa strongly attached itself to the surface of the zona pellucida. When these eggs were cleaned by pipetting them through a small bore pipette and examined with a phase contrast microscope, none contained a spermatozoon either in the perivitelline space or in the vitellus. Clearly no egg of this type had been penetrated by any spermatozoon before the time of recovery from the oviducts. In the other type of eggs, no spermatozoa were found firmly attached to the zona pellucida in spite of their continuous collisions with the zona surface. Examination of these eggs with a phase contrast microscope revealed that each had already been penetrated by a spermatozoon and cortical granule breakdown had taken place. When the two types of eggs were separated and incubated for 1 to 2 h with capacitated spermatozoa, the zona pellucida of already fertilized eggs was not penetrated by extra spermatozoa while unfertilized eggs became highly polyspermic. A series of observations (to be reported in full later) have indicated that the refractory state of the zona pellucida "zona reaction" develops less than 15 min after cortical granule breakdown before sperm chromatin dispersion can be seen. A similar strong zona reaction was

Induction of Zona Reaction in Golden Hamster Eggs by Cortical Granule Material

BRADEN *et al.*¹ have shown that the zona pellucida of mammalian eggs changes after fertilization so that penetration of further spermatozoa is greatly reduced. Austin and Braden² inferred that this zona reaction was due to the action of the

observed throughout the cleavage stages, indicating that this is a permanent change of the zona pellucida.

If cortical granule material is responsible for the development of the zona reaction, it should be possible to induce this reaction by exposing the zona pellucida of an unfertilized egg to the cortical granule material released from fertilized eggs. The following experiments were designed to determine whether this was the case. For each experiment, 200–300 unfertilized eggs were flushed from the oviducts of females injected with human chorionic gonadotropin 14 h previously. After dispersing the cumulus oophorus cells with 0.1% bovine testicular hyaluronidase in Tyrode solution, the eggs were treated with 0.1% hog pancreatic trypsin (1–300, Nutritional Biochem. Corp.) in Tyrode solution containing 0.1% bovine serum albumin fraction V (Armour). Within 1 min the zona pellucida of all eggs was completely digested. After three thorough washings with Tyrode-albumin, the eggs were placed in 25–50 μ l. of incubation medium⁵, under mineral oil, and inseminated with capacitated spermatozoa suspended in 1–2 μ l. of a sperm-capacitating medium (detoxified human serum). Ten minutes later (our preliminary observations had shown us that the cortical granules of such zona-free eggs break down completely with 2.5 to 5 min after insemination), the incubation medium was drawn into a capillary tube (1.4 mm inside diameter), one end of which was then sealed with flame. The tube was centrifuged at 7,000*g* for 10 min, causing eggs and spermatozoa to sediment at the bottom of the tube in a volume of approximately 1 μ l. Supernatant solution (10–25 μ l.) containing cortical granule material was placed under mineral oil in a dish (group A). Also, incubation medium with capacitated spermatozoa but no eggs (that is no cortical granule material) was centrifuged and 10–25 μ l. of the supernatant were placed under mineral oil in another dish (group B). Seven to eighteen fresh unfertilized, cumulus-free eggs in 1–2 μ l. of Tyrode solution were added to each drop. Both groups of eggs were incubated for 30 min at 37° C, then inseminated with capacitated spermatozoa and incubated for an additional hour. Cumulus-free unfertilized eggs and capacitated spermatozoa were mixed concomitantly with 10–25 μ l. of the supernatant solution containing cortical granule material and incubated under mineral oil for 1 h at 37° C (group C). At the end of the incubation, all eggs were examined with both a high power dissecting microscope and a phase contrast microscope for evidence of zona reaction.

The results (Table 1) showed that all eggs of group A displayed a characteristic zona reaction, while none of the eggs of groups B and C showed the reaction. The zona pellucida of every egg in these two groups (B and C) was heavily penetrated by spermatozoa.

Table 1 Effect of Cortical Granule Material in the Induction of the Zona Reaction

Group	Exp. No.	No. of eggs	No. of eggs with zona reaction	No. of eggs without zona reaction
A	1	15	15	0
	2	7	7	0
	3	15	15	0
B	1	10	0	10
	2	7	0	7
	3	18	0	18
C	1	9	0	9
	2	8	0	8
	3	18	0	18

The results reported here clearly demonstrate the zona reaction in hamster eggs and indicate that it is elicited by cortical granule material. The apparent effectiveness of this material in spite of great dilution suggests that the active agent is a very potent substance. The chemical nature of the active agent and the mode of its action on the zona pellucida must await further investigation.

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- 1 Braden, A. W. H., Austin, C. R., and David, H. A., *Austral. J. Biol. Sci.*, **7**, 391 (1954).
- 2 Austin, C. R., and Braden, A. W. H., *J. Exp. Biol.*, **33**, 358 (1956).
- 3 Yanagimachi, R., *J. Reprod. Fertil.*, **18**, 275 (1969).
- 4 Orsini, M. W., *Proc. Animal Care Panel.*, **11**, 193 (1961).
- 5 Biggers, J. D., Whitten, W. K., and Whittingham, D. G., in *Methods of Mammalian Embryology* (edit. by Daniel, J. C.) (Freeman, San Francisco, 1971).
- 6 Barros, C., and Garavagno, A., *J. Reprod. Fertil.*, **22**, 381 (1970).
- 7 Yanagimachi, R., *Biol. Reprod.*, **3**, 147 (1970).

Postnatal Malnutrition and Brain Cholinesterase in Rats

SEVERE malnutrition, especially protein-calorie restriction, early in the life of rats¹ and pigs² can cause behavioural abnormalities which persist long after nutritional rehabilitation. The physiological or biochemical basis for these abnormalities is not known, although long term, irreversible alterations in total DNA³ and brain lipid content have been found to result from early malnutrition. A part of the brain which has been demonstrated to be involved in the control of behaviour is the central cholinergic mechanism⁴, and changes in brain acetylcholinesterase (AChE) and cholinesterase (ChE) have been reported to be correlated with behavioural changes in animals subjected to different environmental situations⁵. Furthermore, Sereni *et al.* found that undernutrition during the first 3 weeks of life decreased the activity of AChE in the brains of rats until they were 2 weeks old. We have now investigated the effects of early protein-calorie restriction on the activity of ChE in the brain.

Pregnant Holtzman rats were fed a purified diet containing 25% casein until the birth of their young. Eight newborn male rats were assigned to each litter and during suckling, lactating dams were divided into an experimental group fed a low protein diet (12% casein) to decrease milk production, and a control group fed on optimum protein diet (25% casein). After a 3 week nursing period, rats on the low milk intake were weaned to a low protein diet (3% casein) for the next 4 weeks. The control group received the optimal protein diet. After the restriction period all animals were fed the optimal diet until they were 38 weeks old.

ChE, DNA and nitrogen in the brain were measured at 9 days, 3 weeks, 7 weeks and 39 weeks of age. The colorimetric method⁶ was used for the ChE assay and nitrogen was determined by the microkjeldahl method. To determine DNA, homogenization of brain tissues followed by hydrolysis of tissue lipids were accomplished as described by Penn and Suwalski⁹. The nucleic acid extracts were obtained by Schneider's procedure¹⁰. Finally, a modified method developed by Burton¹¹ was used for colour development.

Table 1 shows that both specific and total ChE activity increased with age. The rate of increase was most marked during the first 7 weeks of life. At 9 days malnourished rats

Table 1 Brain Cholinesterase Activities of Malnourished Rats and of Rehabilitated Rats

Age (weeks)	Group	ChE/g wet brain weight (R * $\times 10^6$)	Specific activity ChE/100 mg N (R $\times 10^6$)	ChE/mg DNA (R $\times 10^6$)	Total activity ChE/brain (R $\times 10^6$)
1+ (9 days)	Control (8)	3.92 $\pm$ 0.22	28.5 $\pm$ 1.17	5.21 $\pm$ 0.70	3.71 $\pm$ 0.34
	Experimental (8)	4.20 $\pm$ 0.17 †	29.1 $\pm$ 1.29	5.17 $\pm$ 0.44	3.65 $\pm$ 0.20
3	Control (9)	6.92 $\pm$ 0.51	37.9 $\pm$ 2.87	9.16 $\pm$ 1.08	11.1 $\pm$ 0.92
	Experimental (11)	6.61 $\pm$ 0.91	37.1 $\pm$ 5.62	8.28 $\pm$ 1.61	8.43 $\pm$ 1.32 ‡
7	Control (13)	9.51 $\pm$ 0.35	43.0 $\pm$ 1.40	10.5 $\pm$ 0.91	19.0 $\pm$ 1.12
	Experimental (10)	11.8 $\pm$ 0.89 ‡	51.2 $\pm$ 3.18 ‡	10.1 $\pm$ 0.84	15.4 $\pm$ 1.15 ‡
38	Control (5)	8.51 $\pm$ 0.35	47.0 $\pm$ 2.45	7.31 $\pm$ 0.26	16.7 $\pm$ 0.86
	Experimental (5)	9.76 $\pm$ 0.53 ‡	50.7 $\pm$ 3.04 *	8.61 $\pm$ 1.01 *	17.7 $\pm$ 1.32

The numbers of animals in each group are shown in parentheses.

All data are given as mean $\pm$ s.d.

* R is rate in moles substrate hydrolysed per minute.

† Significant difference ($P < 0.05$) from control.

‡ Significant difference ($P < 0.005$) from control.

had an increased specific activity of ChE (ChE/g wet brain weight), the mean difference from the control value being significant at the level of 0.05. When activity was expressed as either ChE/100 mg nitrogen or ChE/mg DNA, there were no differences between the two groups, which does not agree with the report⁷ of lowered brain AChE activity (C μ M/30 m/mgN) in undernourished rats at 6, 8 and 14 days, and no differences at 21 days. At 3 weeks of age the total ChE activity per brain in the malnourished rats was significantly lower ($P < 0.005$) than that of control animals. There were no differences in the specific activity of the enzyme between the two groups, which agrees with Sereni's results on 21 day old rats⁷. On the other hand, Adlard *et al.*¹² reported that the brain AChE activity (per gram wet tissue weight) at 21 days was lower (11%) in rats undernourished during gestation and lactation.

At 7 weeks of age the total enzyme activity per brain in the malnourished group was still lower ($P < 0.005$) than the control value. But the specific activity of the enzyme (ChE/g wet brain weight and ChE/100 mg nitrogen) was significantly increased ($P < 0.005$) in the malnourished group. The specific activity expressed as ChE/mg DNA did not differ between the two groups. The increased specific activity of brain ChE in the 7 week old malnourished rats persisted after the animals had been given a normal diet for 31 weeks. The total enzyme activity per brain did not differ significantly between the two groups at 38 weeks. This is attributable to the smaller brain weights of the previously malnourished rats. Thus, severe protein-calorie deficiency in early life resulted in long lasting increases in the specific activity of brain ChE in rats.

It is interesting that these long lasting increases in brain ChE parallel the long term increases in the emotional reactivity observed in adult rats given identical nutritional treatment¹. Moreover, the long term effects of early handling give similar results in that both emotional reactivity¹³ and sub-cortical ChE¹⁴ decrease.

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- Levitsky, D. A., and Barnes, R. H., *Nature*, **225**, 468 (1970).
- Barnes, R. H., Moore, A. U., and Pond, W. G., *J. Nutr.*, **100**, 149 (1970).
- Winick, M., and Nobel, A., *J. Nutr.*, **89**, 300 (1966).
- Cully, W. J., and Lineberger, R. O., *J. Nutr.*, **96**, 375 (1968).
- Russell, R. W., *Fed. Proc.*, **28**, 121 (1969).
- Rosensweig, M. R., Krech, D., Bennett, E. L., and Diamond, M. C., *J. Comp. Physiol. Psychol.*, **55**, 429 (1962).
- Sereni, F., Principi, N., Perletti, L., and Sereni, L. P., *Biol. Neonat.*, **10**, 254 (1966).
- Ellman, G. L., Courtney, K. D., Andres, jun., V., and Featherstone, R. M., *Biochem. Pharmacol.*, **7**, 88 (1961).
- Penn, N. W., and Suwalski, R., *Biochem. J.*, **115**, 563 (1969).
- Schneider, W. C., *J. Biol. Chem.*, **161**, 293 (1945).
- Burton, K., *Biochem. J.*, **62**, 315 (1956).
- Adlard, B. P. F., Dobbing, J., and Smart, J. L., *Biochem. J.*, **119**, 46P (1970).
- Denenberg, V. H., and Morton, J. R. C., *Animal Behav.*, **12**, 11 (1964).
- Tapp, J. T., and Markowitz, H., *Science*, **140**, 486 (1963).

Stereochemical and Vibrational Theories of Odour

ALTHOUGH many theories of odour specificity have been proposed, only two have been supported by quantitative measurements with statistical analysis. My stereochemical theory correlates the odours of various compounds with the sizes and shapes of their molecular models¹. Wright's vibrational theory relates the odours to certain absorption peaks in their far infrared spectra². It would be interesting to compare the relative success of our two theories directly, but so far this has not been possible, because we had used different test compounds and different statistical criteria.

Accordingly I have decided to apply my theory to the same set of compounds that Wright discussed. These are twenty-six homologues of benzaldehyde and nitrobenzene, originally prepared by Klouwen and Ruys³, who reported their odours in qualitative terminology, for example, bitter almond, weak note of sassafras. As a basis for comparing our theories, however, it would be preferable to obtain quantitative measurements of the degree to which each compound resembles in odour a single standard, for example, benzaldehyde itself (which is the principal constituent of bitter almond oil).

For this purpose I used a panel of twenty-nine observers who were experienced in olfactory tests but unfamiliar with most of these compounds⁴. About 0.1 g of each odorant was presented in coded 10 ml. glass-stoppered Erlenmeyer flasks, for comparison with a standard flask containing 0.1 g

benzaldehyde. The observers were instructed to ignore any differences in intensity, and to concentrate on odour quality. The scale of odour similarity runs from 0 for no similarity up to 8 for extreme similarity. The odorants were presented in daily batches of eight in randomized sequence, and each odour appeared twice. Hence the odour similarities given in the middle column of Table 1 represent the averages of more than fifty individual judgments. These odour similarity values constitute the experimental olfactory data which each theory must seek to explain.

The starting point for my stereochemical theory is a set of three orthogonal silhouette photographs of a scale model of the benzaldehyde molecule. Using the PAPA scanning computer¹, the molecular silhouette of each homologous compound was compared with the benzaldehyde silhouette as the standard. The centre of gravity of each molecular silhouette was taken as the datum point. The computer-generated scale of molecular shape resemblance runs from 120 for complete identity down to 90 or less for very dissimilar molecules. The detailed results are given at the right of Table 1.

Table 1 Olfactory and Stereochemical Similarities to Benzaldehyde

Compound	Odour similarity *	Molecular shape †
Benzaldehyde (standard)	(7.17) ‡	(120.6) ‡
<i>o</i> -Methylbenzaldehyde	3.79	109.0
<i>m</i> -Methylbenzaldehyde	4.35	106.8
<i>p</i> -Methylbenzaldehyde	3.62	108.7
<i>o</i> -Ethylbenzaldehyde	2.98	103.1
<i>m</i> -Ethylbenzaldehyde	1.34	101.6
<i>p</i> -Ethylbenzaldehyde	2.98	102.5
<i>o</i> -Isopropylbenzaldehyde	—§	102.7
<i>m</i> -Isopropylbenzaldehyde	1.54	101.0
<i>p</i> -Isopropylbenzaldehyde	1.44	100.3
<i>o</i> -tert. Butylbenzaldehyde	0.84	100.8
<i>m</i> -tert. Butylbenzaldehyde	1.40	97.5
<i>p</i> -tert. Butylbenzaldehyde	1.25	97.1
Nitrobenzene	6.40	112.5
<i>o</i> -Methylnitrobenzene	3.87	108.1
<i>m</i> -Methylnitrobenzene	5.76	105.2
<i>p</i> -Methylnitrobenzene	4.36	107.5
<i>o</i> -Ethylnitrobenzene	1.69	103.3
<i>m</i> -Ethylnitrobenzene	1.73	101.1
<i>p</i> -Ethylnitrobenzene	2.17	102.6
<i>o</i> -Isopropylnitrobenzene	1.20	101.5
<i>m</i> -Isopropylnitrobenzene	1.09	99.0
<i>p</i> -Isopropylnitrobenzene	1.59	100.5
<i>o</i> -tert. Butylnitrobenzene	0.81	98.8
<i>m</i> -tert. Butylnitrobenzene	1.31	99.7
<i>p</i> -tert. Butylnitrobenzene	1.27	98.3
Benzonitrile	6.29	112.0

* Similarity to odour of benzaldehyde.

† Resemblance to molecular silhouette of benzaldehyde.

‡ Omitted from statistical calculations.

§ Sample unobtainable.

These data are easier to comprehend in the form of a graph (Fig. 1). Clearly there is a strong correlation between odour quality and molecular shape. The more closely a compound resembles benzaldehyde in the shape of its molecule, the more similar it is to benzaldehyde in odour. (The only exception is *m*-methylnitrobenzene which smelled much closer to benzaldehyde than its molecular shape would indicate.) The pair of regression lines diverge mildly, indicating a relatively high correlation coefficient, $r=0.90$. With $N=25$ compounds, the probability was extremely low that this correlation could be fortuitous, $P<0.001$. The molecular silhouette photographs were also run through the PAPA computer a second time, using the functional group (CHO or NO₂) of the molecular silhouette as datum point instead of the centre of gravity. The results were not quite so good, $r=0.71$, but the probability level remained highly significant, $P<0.001$. The experimental data evidently provide strong support for the stereochemical theory of odour.

Now it is the turn of the vibrational theory to explain the observed olfactory values for this series of compounds. In

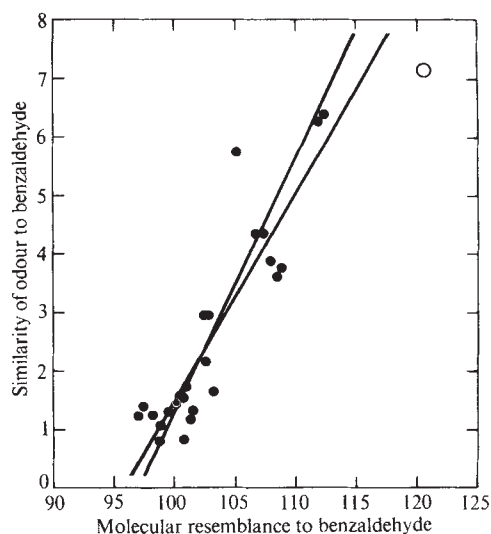


Fig. 1 The almond odour is correlated with molecular shape for 25 homologues of benzaldehyde and nitrobenzene. $r=0.90$. (Two regression lines are shown because both variables are subject to experimental error; the open circle represents the benzaldehyde standard.)

the study reported earlier², Wright and Robson did not have quantitative olfactory data available, so they treated the sixteen almond-like odorants as a group, contrasting them with an external control group of thirty-one miscellaneous compounds with other odours. Thus there is no basis on which to compare their χ^2 analysis with my regression analysis, even though their final probability levels appear much the same as mine.

If our two theories are to be realistically compared, it is necessary for Wright to devise a method for computing, from the infrared spectral frequencies, a quantitative measurement of how closely the spectrum of each compound resembles the spectrum of benzaldehyde itself, or some composite spectrum based on several compounds having an almond odour. This procedure would enable him to add his column of vibrational data to Table 1, and draw his graph corresponding with Fig. 1. Then, and only then, could we compare correlation coefficients, and obtain a direct measure of which theory fits the olfactory data more precisely.

I have developed one aspect among three quantitative comparisons of our theories that are in progress⁵. I thank Dr F. Rijkens of the Chemische Fabriek "Naarden" who supplied most of these compounds, and Drs R. H. Wright and J. R. Hughes for samples. Many of the aldehydes were freshly regenerated here from their semicarbazones by Dr Marvel-Dare Nutting. The measurements with the PAPA computer were furnished by Miss Silvana Vallergera at the University of Genoa.

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¹ Amore, J. E., Palmieri, G., and Wanke, E., *Nature*, **216**, 1084 (1967).

² Wright, R. H., and Robson, A., *Nature*, **222**, 290 (1969).

³ Klouwen, M. H., and Ruys, A. H., *Parfum. Cosmét. Savons*, **6**, 6 (1963).

⁴ Amore, J. E., and Venstrom, D., *J. Food Sci.*, **31**, 118 (1966).

⁵ Amore, J. E., in *Gustation and Olfaction* (edit. by Ohloff, G., and Thomas, A. F.), 147 (Academic Press, London, 1971).

Evidence for Inhibition by Brain Serotonin of Mouse Killing Behaviour in Rats

THERE is some doubt about the role of brain serotonin in aggression. In mice made aggressive by prolonged isolation, there is a decrease in brain serotonin turnover¹, while increased aggressiveness has been noted in cats² and rats³ given *p*-chlorophenylalanine (PCPA), which inhibits serotonin synthesis⁴, although the experiments with cats² were not confirmed later⁵. Dominguez and Longo⁶ reported that PCPA attenuates irritability and viciousness in rats with septal lesions, and Welch and Welch⁷ observed decreased aggressiveness in isolated mice given PCPA. The explanation for these contradictory results may be that different models of aggression are not mediated through the same areas of the brain or by the same biochemical mechanisms.

We have therefore investigated the effect of modifying brain serotonin metabolism on mouse killing behaviour induced in isolated rats with or without lesions in the anterior olfactory area^{8,9}. Two operations were performed on male Wistar rats (200–240 g, Morini). In the first (bulbectomy), the cortex of the olfactory bulb and the anterior part of the olfactory nucleus were removed on both sides of the brain. A glass cannula was inserted at a right angle to the lamina cribrosa of the ethmoid bone through a hole in the frontal bone. In the second operation (bulbotomy) both olfactory stalks were cut with a needle inserted by the same route as the cannula. Cuts were caudal to the olfactory nucleus, so that most of the rostral end of the olfactory tubercle was sectioned. Sham operations were performed by inserting a needle as in the other two cases but without sectioning the olfactory stalk.

After surgery, rats were isolated in cages (26 × 20 × 14.5 cm) in a regime of light from 0700 to 1900 h. Mouse killing activity was tested after 10–15 days by putting a white male mouse into the rat's home cage. We recorded the number of rats which killed their mouse within 1 h unless otherwise stated. All tests were made at 2000 h. Killing was minimal in the middle of the dark or light period.

Table 1 Induction by PCPA of Mouse Killing Behaviour in Intact and Olfactory Bulb-Lesioned Rats

Rats	No. of mouse-killers/No. of animals treated		No. of animals treated	
	Saline	PCPA	Saline	PCPA
Intact	6/50	18/40	<0.02	
Sham operated	2/25	8/20	<0.02	ns
Bulbectomized	4/40	34/40	<0.001	<0.01
Bulbotomized	28/40	35/35	<0.001	<0.001

DL-PCPA methylester HCl was injected intraperitoneally as a single dose of 400 mg/kg, 36 h before tests for mouse killing behaviour.

Tests were performed at the tenth day of isolation.

For statistical analysis chi-square test was used.

* Compared with the correspondent intact group.

† Compared with the correspondent group injected with saline.

The behaviour of the sham operated rats did not differ significantly from that of untreated controls. After 10 days of isolation, about 10% of rats in both groups were killing mice (Table 1). PCPA increased the percentage of mouse killers to 45% and 50% in the control and in the sham operated group, respectively. Contrary to previous results¹⁰, bilateral removal of the olfactory bulb by suction induced no muricidal behaviour. PCPA, however, caused 85% of these rats to kill mice.

Sectioning the olfactory stalk produced 70% of mouse killers and PCPA increased this to 100%. Moreover, treatment with PCPA always induced killing in rats which had not been affected by bulbectomy (Table 1).

In about 30% of these animals mouse killing induced by PCPA ceased within 10 days of the last administration of the drug; once established in the remaining animals it persisted indefinitely, but the killing response occurred after a longer

Table 2 Inhibition by 5-HTP and Pargyline of Mouse Killing Behaviour

Treatment	No. of killers*/No. of animals treated		P†
	Intact	Bulbotomized	
Saline	38/38	25/25	
5-HTP	10/20	16/28	<0.001
Pargyline	7/30	6/28	<0.001

Drugs were injected intraperitoneally; L-5-HTP at a dose of 30 mg/kg 20 min before test; pargyline at a dose of 100 mg/kg 8 h before test.

* Animals who killed the mouse within 15 min from presentation.

† Compared with the correspondent saline-injected group.

latency. Since PCPA, in addition to its effect on serotonin, produces a small but significant depletion of brain catecholamines⁴, we investigated the effect of selective inhibition of catecholamine synthesis on mouse killing. Ten non-killer, bulbectomized rats were treated with D,L- α -methyl-tyrosine, methyl ester (HCl 250 mg/kg given intraperitoneally) and tested for mouse killing behaviour at 6 h intervals for 24 h. None of these animals turned into mouse killers.

Since these experiments suggested that mouse killing was potentiated by selective depletion of brain serotonin, we investigated the effect of increasing this substance by treatment with L-5-hydroxytryptophan (5-HTP), its immediate precursor, or pargyline, a monoamine oxidase inhibitor. 5-HTP (30 mg/kg) or pargyline (100 mg/kg) were administered intraperitoneally 20 min and 8 h respectively, before the test, to rats which had become killers as a result of isolation or bulbectomy.

As Table 2 shows, 15 min after the mouse had been presented, only 50% of the animals treated with 5-HTP and 23% of those treated with pargyline killed the mouse. Moreover, none of the pargyline-treated animals, which had killed the mouse 12 h after presentation, ate their victims, although before treatment they ate up part or all of the mouse within 1 h.

Our studies support the idea that inhibitory influences on the mouse killing by rats originate in the anterior olfactory area⁹. Our findings may differ from those of Vergnes and Karli¹⁰ because they removed the olfactory bulb by inserting a suction cannula in a fronto-caudal direction. This could have removed a more extensive portion of the olfactory nucleus and/or part of the olfactory tubercle.

Since in our experiments bulbectomy eliminated all the olfactory cortex but only the anterior part of the olfactory nucleus, which in turn is eliminated after bulbotomy, these results suggest that the olfactory nucleus is involved in the inhibition of mouse killing. Some inhibitory influences, however, must originate within the olfactory bulb for PCPA produced many more killers among bulbectomized than sham operated animals.

The fact that either bulbectomy or PCPA alone induced mouse killing in only a percentage of animals, while the combination of the two was effective in 100% of cases, suggests that the two procedures act through different mechanisms. PCPA probably removes serotonergic inhibitory influences. This idea is supported by the finding that mouse killing is antagonized or suppressed in animals whose brain serotonin has been increased by 5-HTP or pargyline.

The fact that PCPA potentiates mouse-killing behaviour in rats but antagonizes other types of aggressiveness, indicates that the former is not only behaviourally¹¹ but also biochemically different from the others.

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- ¹ Giacalone, E., Tansella, M., Valzelli, L., and Garattini, S., *Biochem. Pharmacol.*, **17**, 1315 (1968).
- ² Ferguson, J., Henriksen, S., Cohen, H., Mitchell, G., Barchas, J., and Dement, W., *Science*, **161**, 499 (1970).
- ³ Sheard, M. H., *Brain Res.*, **15**, 524 (1969).
- ⁴ Koe, B. K., and Weissman, A., *J. Pharmacol. Exp. Therap.*, **154**, 499 (1966).
- ⁵ Zitrin, A., Beach, F. A., Barchas, J. D., and Dement, W. C., *Science*, **170**, 868 (1970).
- ⁶ Dominguez, M., and Longo, V. G., *Physiol. Behav.*, **4**, 1031 (1969).
- ⁷ Welch, A. S., and Welch, B. L., *Biochem. Pharmacol.*, **17**, 699 (1968).
- ⁸ Vergnes, M., and Karli, P., *CR Soc. Biol.*, **163**, 2704 (1969).
- ⁹ Vergnes, M., and Karli, P., *CR Soc. Biol.*, **159**, 972 (1965).
- ¹⁰ Vergnes, M., and Karli, P., *CR Soc. Biol.*, **157**, 1061 (1963).
- ¹¹ Barnett, S. A., in *Aggressive Behaviour* (edit. by Garattini, S. E., and Sigg, E. B.), 3 (Excerpta Medica Foundation, Amsterdam, 1969).

Retraction and Expansion of the Dendritic Tree of Motor Neurones of Adult Rats induced *in vivo*

Cerf and Chacko¹ found that the number and diameter of motor neurone dendrites decreased after axotomy in the adult bullfrog. The purposes of the investigation reported here are to confirm and extend these observations in a mammal, to determine whether the dendritic changes are due to loss of axoplasm or loss of effective neuromuscular transmission, to see if dendrites recover after nerve regeneration has restored contact between the nerve and the originally innervated muscle mass, and if they recover after the regenerating nerve makes contact with different denervated muscle. Quantitative assessments of the dendritic spread of developing rat and human cortical neurones have been made by Eayrs and Goodhead², and Schadé and van Groenigen³. Schapiro and Vukovitch⁴ found that the number of dendritic spines of rat cortical neurones could be influenced by variety of experience during

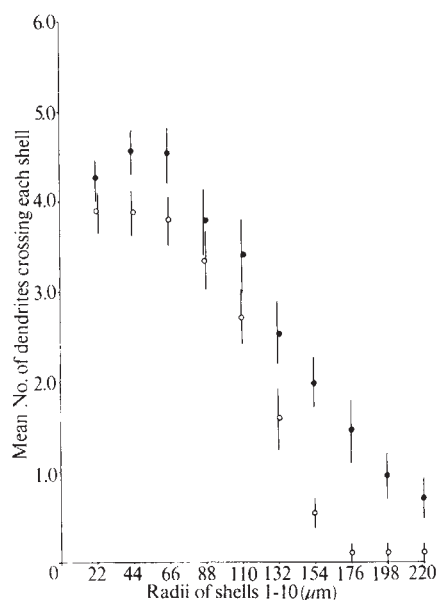


Fig. 1 The number of dendrites crossing concentric shells is expressed graphically against the radius of the shell. ●, Normal right side; ○, treated left side of a rat 6 weeks after botulinum toxin. Twenty neurones were measured on each side and the mean values are shown, with standard errors.

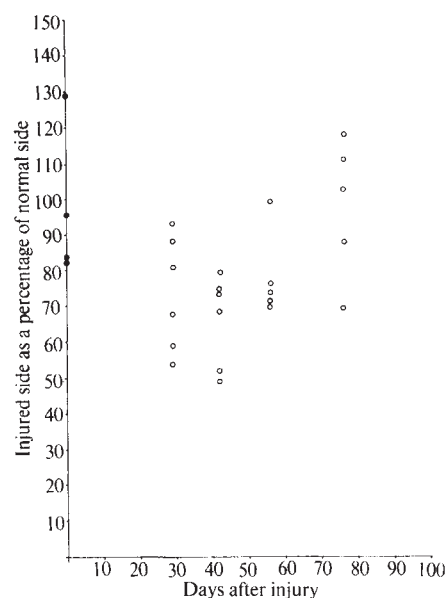


Fig. 2 Dendritic spread of hypoglossal neurones is expressed against the time since hypoglossal nerve transection; each point was obtained from twenty normal and twenty abnormal neurones of one rat. Values found between 4 weeks and 8 weeks after injury are significantly different from normal ($P < 0.02$; > 0.01). ●, Normal rats; ○, experimental rats.

development; Jones and Thomas⁵ and Matthews and Powell⁶ reported a decrease in the number and length of dendrites after deafferentation. Possible neuronal plasticity in the central nervous system of adult rats has been reported by Raisman⁷.

Sholl's⁸ "concentric shell" method was used to assess the dendritic fields of hypoglossal neurones in rat medullae prepared by the Golgi-Cox⁹ method with 6 weeks' impregnation. Two hundred μ m lateral ventricular nucleus transverse and coronal sections were used to measure the dendritic spread of twenty impregnated neurones from the left and from the right hypoglossal nucleus of each rat; the mean number of dendrites crossing each of the concentric shells was determined and expressed graphically against the radius of the shell. Fig. 1 shows mean values obtained from the normal right hypoglossal nucleus, and from the left hypoglossal nucleus 6 weeks after injecting botulinum toxin into the left side of the tongue (experimental series 2, see below). The smaller number of dendrites found passing through peripheral "shells" after injection suggests that terminal dendrites have retracted or degenerated, or failed to impregnate in these neurones. The mean dendritic spread of neurones of one hypoglossal nucleus was expressed by summing all the means from shell 2 to shell 10 (Fig. 1); this sum of means obtained from the nucleus containing the abnormal neurones was then calculated as a percentage of the sum of means obtained similarly from neurones of the opposite, unoperated, nucleus. Only in this way could variation between rats in impregnation by the Golgi-Cox method be overcome. No correction was made for planar projection of the dendrites which showed no preferred plane of radiation.

There were five experimental series of male rats aged 3 months, running in part concurrently. In the first, the left hypoglossal nerve was transected at the point where it crossed the carotid bifurcation⁹, and regeneration proceeded; in the second, 250 μ g of botulinum toxin type A was injected into the left half of the tongue¹⁰ to prevent local neuromuscular transmission; in the third, the left hypoglossal nerve was transplanted into the normally innervated left sternomastoid¹¹; in the fourth, the left hypoglossal nerve was transplanted into

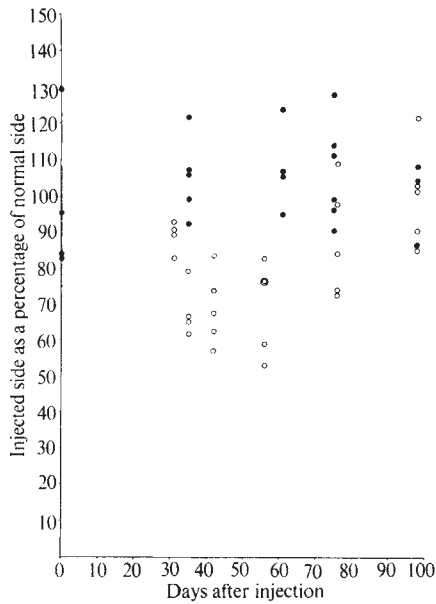


Fig. 3 Dendritic spread of hypoglossal neurones is expressed against the time since injection of botulinum toxin; each point was obtained from twenty normal and twenty abnormal neurones of one rat. ●, Normal rats, and those injected with toxin previously subjected to prolonged boiling; ○, rats injected with toxin. Values found between 5 weeks and 8 weeks after injection are significantly different from normal and control rats ($P < 0.001$). No significant change is found in rats injected with boiled toxin.

the normally innervated left sternomastoid, and 70 days later the spinal accessory nerve innervating this muscle was transected and diverted¹¹, permitting the previously implanted hypoglossal nerve to innervate this "foreign" muscle; in the fifth, the procedure of the fourth series was followed, but the afferent nerves from the sternomastoid passing to the cervical plexus were transected at the same time as the spinal accessory.

Nerve injury (experimental series 1) caused a reversible decrease in dendritic spread (Fig. 2) closely resembling that found after botulinum toxin (experimental series 2) (Fig. 3). This suggests that the changes are not due to loss of axoplasm or to direct cell injury as such. The re-expansion of dendritic field found between the fifth and tenth weeks need not be due to re-establishment of functional contact between nerve and muscle but could be due to intrinsic late neuronal events; but, if regeneration is impeded by surrounding the transected nerve with normally innervated muscle (experimental series 3) no dendritic re-expansion occurs at this time (Fig. 4). The neurones of the implanted nerve remain quite capable of responding, however, for if this muscle is then denervated (experimental series 4), the dendritic fields grow out (Fig. 5). This late induced expansion is therefore independent of the initial loss of neuromuscular contact, but depends directly on its re-establishment and shows no specificity for the muscle originally innervated. Combined sensory and motor denervation of the recipient muscle (experimental series 5) did not influence the response obtained from motor denervation alone.

It therefore seems that dendritic retraction is due to loss of effective neuromuscular contact and that re-expansion is a direct consequence of re-establishment of such contact; failure of sensory denervation of the recipient muscle to influence the response obtained with muscle denervation alone does not show definitely that the effect is mediated to the perikaryon by the axon involved, for too many unaffected collateral sensory pathways exist which could convey information about sternomastoid contraction.

Preliminary results obtained from ultrastructural stereology indicate that the volume of hypoglossal dendrites changes in a manner closely corresponding to the dendritic field alterations

reported here. It is therefore unlikely that the results indicate altered neuronal impregnation by the Golgi-Cox method in these circumstances.

The fate of preganglionic synaptic endings originally terminating on dendrites which are then caused to retract by axonal events, and the origin of synaptic endings appearing on re-expanded dendrites, are currently under investigation.

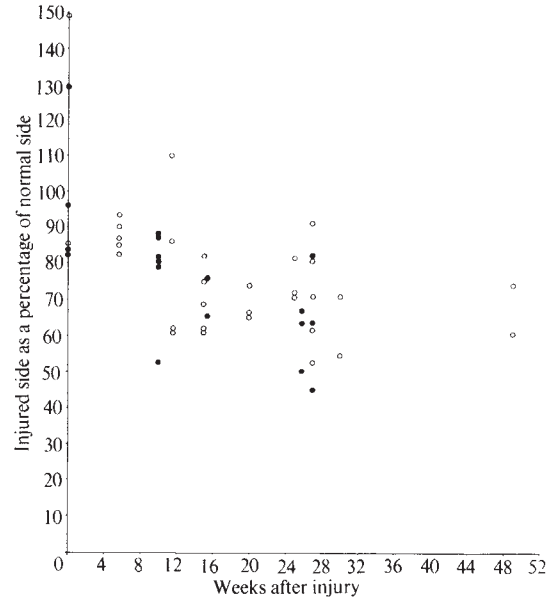


Fig. 4 Dendritic spread of hypoglossal neurones is expressed against the time since implanting the hypoglossal nerve into the normally innervated sternomastoid. ●, Transverse sections of medulla; ○, coronal sections of medulla. Note the failure of the dendritic tree to re-expand after the sixth week. The retraction is statistically significant ($P < 0.02$; > 0.01).

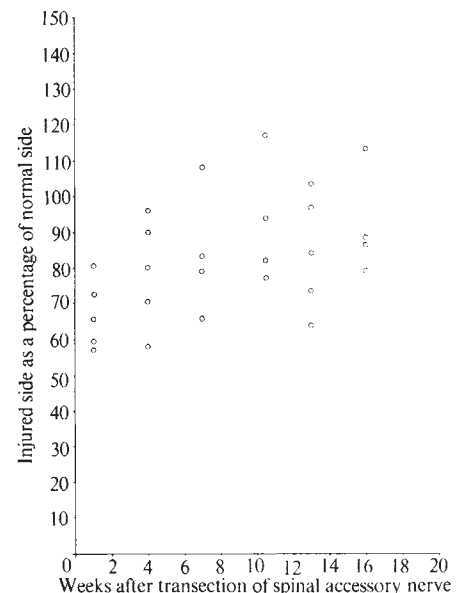


Fig. 5 Dendritic spread of hypoglossal neurones is expressed against the time since transection of the spinal accessory nerve 10 weeks after implanting the hypoglossal nerve in the sternomastoid. Zero time therefore corresponds to the 10 week point on Fig. 4. The dendritic field re-expands, and the points between 7 and 16 weeks after transection of the spinal accessory nerve are significantly different from those in the comparable period in Fig. 4, where the spinal accessory was not transected ($P < 0.01$; > 0.001).

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- ¹ Cerf, J. A., and Chacko, L. W., *J. Comp. Neurol.*, **109**, 205 (1958).
- ² Eayrs, J. T., and Goodhead, B., *J. Anat.*, **93**, 385 (1959).
- ³ Schädé, J. P., and van Groenigen, W. B., *Acta Anat.*, **47**, 74 (1961).
- ⁴ Schapiro, S., and Vukovitch, K. A., *Science*, **167**, 292 (1970).
- ⁵ Jones, W. H., and Thomas, D. B., *J. Anat.*, **96**, 375 (1962).
- ⁶ Matthews, M. R., and Powell, T. P. S., *J. Anat.*, **96**, 89 (1962).
- ⁷ Raisman, G., *Brain Res.*, **14**, 25 (1969).
- ⁸ Sholl, D. A., *J. Anat.*, **87**, 387 (1953).
- ⁹ Watson, W. E., *J. Physiol.*, **180**, 741 (1965).
- ¹⁰ Watson, W. E., *J. Physiol.*, **202**, 611 (1969).
- ¹¹ Watson, W. E., *J. Physiol.*, **210**, 321 (1970).

Fluid Balance in the Sahara

It has often been said that the inhabitants of arid regions seem to require far less fluid than non-indigenous visitors¹, but opinions vary as to whether this discrepancy is attributable to ethnic differences in the inherent physiological mechanisms which are concerned with sweating and water metabolism, or to cultural-adaptive factors such as diet, clothing and mode of activity²⁻⁴. In an attempt to elucidate this problem, I and a Toureg nomad undertook a 500 mile journey in the Central Sahara, and I present here the observations which were made of our respective fluid requirements during this trek.

To ensure that the physiological observations were directly comparable, I chose clothing that was similar in style and colour to that of my companion and I followed an identical régime of activity. We were alike in age and physique and shared the same food supply. In this way external factors which might have affected water requirements had the same influence in both of us. In the following account I refer to myself as subject 1 and to the nomad as subject 2.

Subject 1 was a member of the 1970 British Air Mountain Expedition which carried out a multidisciplinary research programme in the southern Sahara. At the start of the journey he had already been in the desert for more than 2 months and was therefore fully acclimatized. At this time he was 35 yr old, weighed 73.25 kg and was 1.79 m tall. Subject 2 was a 38 yr old Toureg nomad of the Kel Etedale clan. Although measurements were not obtained, he was similar in height, weight, physique and body fat to subject 1.

The journey began in March 1970 in the village of Iferouane (latitude 19° 20' N; longitude 8° 10' E) and ended in the oasis of Tamanrasset (latitude 22° 50' N; longitude 5° 20' E), in the Ahaggar Massif of southern Algeria. Water and food were carried on two camels and two others were ridden intermittently. The 500 mile trek was completed in 20 days at an average rate of 25 miles per day.

Snacks were taken at dawn and noon, with a more substantial meal after sunset. Both subjects shared a mixed diet of millet, dried cheese and British Army "compo" rations. Their average 24 h intake was 2,200 calories, composed of 280 g of carbohydrate, 80 g of protein and 85 g of fat. Salt was added liberally to the food. Subject 2 preferred millet to meat,

and it is likely therefore that his protein intake was somewhat less than that of his companion.

The water camel carried four 32 pint plastic jerry cans and a goat skin which held 42 pints. Wells were encountered intermittently throughout the journey and water was only once in short supply. On this occasion (day 12) an isolated well was found to be blocked by a dead camel. This emergency was resolved by a 4 day forced march to the next water point.

To facilitate the accurate measurement of fluid intake, all water for drinking was decanted into the subject's personal water bottle. A record was kept of the number of bottles of water which were consumed in each 24 h from sunrise to sunrise. The quantity of drinking water remaining in a bottle at the end of the 24 h was measured, and because the total volume of the bottles was known, the individual's fluid intake could be calculated.

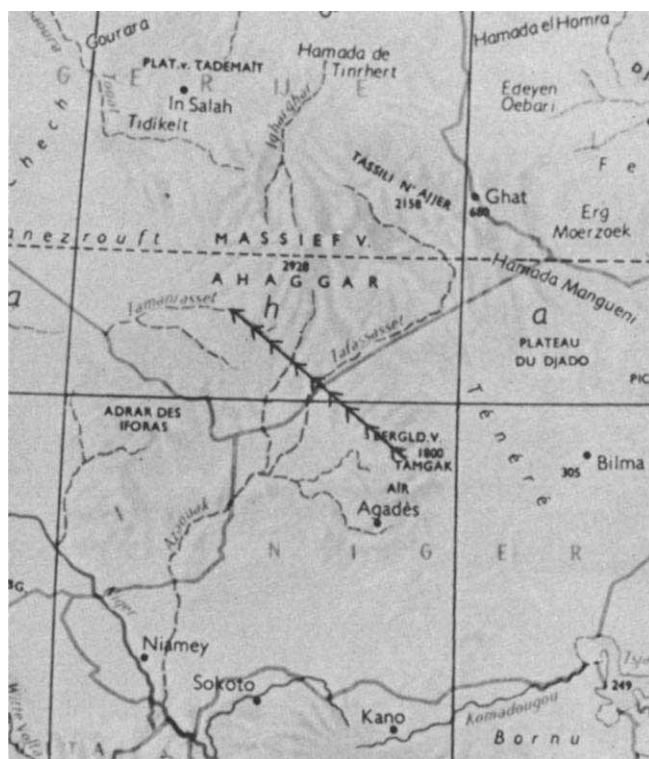


Fig. 1 The route that was followed from Iferouane in the Air to Tamanrasset in the Ahaggar.

Apart from the 4 day period of water shortage, water was never rationed. The subjects were always able to drink their fill, according to the dictates of their thirst. It was noticeable that a considerable proportion of the water intake was drunk in the evenings when the day's march was over. The first morning specimen of urine was collected in a graduated plastic container and the volume and specific gravity were recorded. The Fantus test was carried out on this specimen to demonstrate the presence of urinary chloride. Because of the language barrier, attempts at collection of the total urine output met with incredulity and hilarity and this aspect of the investigation was therefore regrettably abandoned.

The daily fluid intakes are shown in Table 1, together with the early morning urine volumes and specific gravities. The distances covered and the noon air temperature and humidity, as measured by a whirling hygrometer, are also recorded.

The usual 24 h fluid requirements varied from 4 to 8 l. and were very similar in both subjects. The Toureg's intake was usually lower than that of subject 1, but only by a factor of a few per cent.

On day 9, which was largely spent in watering the camels and collecting fodder, the fluid intake was reduced. On days 13 14

Table 1 Daily Observations

Day	24 h fluid intake (l.)		Early morning urine		Noon meteorological data			Estimated distance covered (km)		
			Volume (ml.)	Specific gravity	Dry bulb	Wet bulb	Relative humidity			
	Subject 1	Subject 2	Subject 1	Subject 2	Subject 1	Subject 2	°C		°C	
1							32.4	18.1	23	22
2	4.5	4.1	520	490	1,012	1,008	31.8	16.7	19	58
3	5.3	5.0	410	500	1,008	1,010	32.0	16.9	24	62
4	4.6	4.8	670	610	1,020	1,008	32.0	16.9	24	58
5	6.1	5.6	610	670	1,012	1,012	32.3	17.2	19	68
6	6.2	5.9	470	520	1,014	1,012	32.9	17.0	29	57
7	6.1	5.8	250	670	1,022	1,008	33.4	17.1	16	66
8	8.0	7.2	480	610	1,008	1,010	37.0	17.3	9	56
9	3.1	3.0	600	420	1,008	1,012	36.1	16.8	9	26
10	7.9	7.6	560	890	1,010	1,004	37.0	17.1	9	61
11	7.7	7.8	610	390	1,014	1,012	39.1	19.4	12	58
12	7.9	7.5	490	410	1,012	1,008	40.2	19.4	14	66
13	4.0	4.0	470	610	1,012	1,008	40.9	19.8	11	62
14	2.2	2.2	210	190	1,028	1,030	39.8	19.4	11	87
15	—	—	180	160	1,038	1,036	39.7	19.2	11	83
16	13.6	15.1	—	—	—	—	35.9	17.0	10	20
17	9.1	7.8	340	700	1,016	1,012	36.2	17.1	16	56
18	7.7	7.0	640	540	1,008	1,010	35.1	17.1	12	59
19	6.4	6.4	510	610	1,012	1,012	32.8	17.2	18	62
20	4.5	4.1	570	620	1,008	1,014	32.2	17.2	19	47

and 15, because of the blocked well, the remaining water supplies were rationed and shared. The fluid intake on day 16, when the next water point was reached, was almost double that of the normal day. The subjects were probably still somewhat dehydrated on day 17, as their fluid intakes on this day remained high.

The urinary specific gravity observations and the morning urine volumes were within expected limits and indicated that

the subjects were not in a state of chronic dehydration. Similarly, the tests for urinary chloride showed that they were never salt deficient.

At an early stage of the journey it became obvious that the Toureg's mode of activity was optimal for life and survival in the desert. He avoided heat gain by spending the hottest part of the day in rest, by riding the camels during the moderately hot period and by walking during the relatively cool morning and evening. Similarly, his traditional dress was efficient in the prevention of heat gain from solar radiation and the facilitation of heat loss by convection.

The Toureg's diet was comparatively low in calories and moderately low in protein. A diet of this nature requires a relatively limited quantity of water for metabolism and it would prevent the accumulation of body fat and thus facilitate heat exchange. It must be stressed that this diet was eaten from choice rather than from any lack of food. In this context, it is of interest that the members of the Toureg race tend to be very tall, thin and angular in physique⁵. This somatic configuration facilitates heat loss from the body in a hot dry environment. (Compare the short fat Eskimo who is equally well adapted to the conservation of heat.)

Although water was frequently consumed during the heat of the day, a considerable proportion of the daily intake of fluid was drunk in the evening. This is in accordance with the observations of Adolph⁶ and of Leithead and Pallister⁷, who suggested that in a hot environment a water deficit of several litres could be accumulated, without noticeable effect on the thirst.

In my investigation the two subjects were alike in age, height, weight and physique. During the journey their pattern of activity, diet and clothing were almost identical. For this reason all the external factors which might have influenced their water metabolism were directly comparable. Their 24 h fluid intakes were very similar; that of the Toureg was consistently minimally lower than that of the Englishman.

These findings suggest that the fundamental water requirements of a Toureg nomad and an acclimatized European are the same. Discrepancies which have been observed previously have probably been caused by cultural-adaptive factors rather than by any difference in basic physiological mechanisms.



Fig. 2 Toureg of the southern Sahara.

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¹ Kirmiz, J. P., *Adaption to Desert Environments*, 88 (Butterworths, London, 1962).

² Kuno, Y., *Human Perspiration*, 416 (Thomas, Springfield, Illinois, 1956).

³ Edholm, O. G., and Bacharach, A. L., *Exploration Medicine*, 211 (Wright, Bristol, 1965).

⁴ Wyndham, C. H., et al., *J. Appl. Physiol.*, **19**, 885 (1964).

⁵ Lhote, H., *Le Sahara-désert Mystérieux*, 127 (Bourrellier, Paris, 1937).

⁶ Adolph, E. F., in *Physiology of Heat Regulation*, 330 (W. B. Saunders, Philadelphia, 1949).

⁷ Leithead, C. L., and Lind, A. R., *Heat Stress and Heat Disorders*, 143 (Cassell, London, 1964).

Experimental Study of Mimicry in Salamanders involving *Notophthalmus viridescens viridescens* and *Pseudotriton ruber schencki*

THE aposematically coloured eft stage of the red spotted newt, *Notophthalmus viridescens viridescens*, is both toxic and noxious to potential predators. The black chinned red salamander, *Pseudotriton ruber schencki*, is the most common red-orange salamander other than eft *N. v. viridescens* in the vicinity of the Highlands Biological Station in North Carolina. This study provides experimental evidence that *P. r. schencki* is a Batesian mimic of *N. v. viridescens*.

Because wild potential avian predators were found to be partially preconditioned to avoid both efts (model) and *P. r. schencki* (mimic), feeding experiments were carried out with domestic chickens to determine the behaviour of naive birds¹. The chickens were maintained collectively in an outside enclosure but were individually isolated for testing. Each trial involved either a model or a mimic and a non-mimic salamander; this procedure is similar to that used by Brower². Drab coloured, palatable salamanders of the genus *Desmognathus* (*D. monticola*, *D. quadramaculatus*) were used as controls (non-mimics). The feeding order of birds, the size of the salamanders, and the order of presentation of the controls were all randomized; two trials were made each day.

Seven chickens were divided into two experimental groups. The four chickens in group I were each offered ten models; after conditioning (consistent visual rejection of models), six mimics were offered to each chicken. The three chickens of group II were each offered six mimics before and after presentation of ten models. For both groups, the trials were discontinued for 7 days, after which mimics were again offered to each chicken. All the salamanders used in the trials were anaesthetized in separate solutions of 'Finquel' (tricaine methanesulfonate) to eliminate behavioural differences or similarities; they were offered to chickens dorsal side up in a shallow plastic dish. Each chicken was allowed 5 min to react to each salamander, and its response (characterized as: eaten, pecked

but not eaten, and not touched) was measured to the nearest second.

The chickens in group I initially seized, but did not eat, the noxious model; rejection of models on sight, without contact, occurred after one to seven trials ($\bar{x}=5$). All the chickens in group I rejected all mimics after conditioning with efts (Table 1). Before conditioning with models the chickens in group II ate all mimics offered and seized, but did not eat, the first models presented; models were not touched after two to five trials ($\bar{x}=3.3$). After conditioning with ten efts, two chickens in group II rejected all mimics; one chicken ate all mimics (Table 1). The mean seizure times for the latter chicken were as follows: mimics before conditioning with models, 2.0 s; mimics after conditioning with models, 3.5 s; non-mimics, 1.3 s. When models were again offered to this chicken, one was pecked and five were not touched; when offered, mimics were again eaten. All non-mimics, *Desmognathus*, were eaten rapidly by all chickens, the mean seizure

Table 1 Responses of Chickens to Model, Mimic and Non-mimic Salamanders

	Non-mimic	Model	Mimic before conditioning	Mimic after conditioning
Group I				
Eaten	83	1		0
Pecked, not eaten	0	13		1
Not touched	0	26		42
Group II				
Eaten	86	0	18	8*
Pecked, not eaten	0	11	0	1
Not touched	0	25	0	23

* All eaten by one chicken.

time ranging from 1.3 to 15.7 s for individual chickens in both groups. The mean seizure time for mimics eaten ranged from 2.0 to 22.2 s for group II. Analysis of variance revealed no significant differences in seizure times.

Statistically significant differences exist between number of mimics eaten after conditioning and number of non-mimics eaten during the same trials: group I, $\chi^2(3 \text{ d.f.})=43.0$, $P<0.005$; group II, $\chi^2(2 \text{ d.f.})=24.0$, $P<0.005$. There was no difference between the number of mimics and non-mimics eaten before conditioning ($\chi^2=0$). We conclude that the palatable mimic (*P. r. schencki*) derives a selective advantage over non-aposematically coloured salamanders due to their similarity in colour to the noxious model (*N. v. viridescens*), and that this relationship is clearly the result of Batesian mimicry.

We thank S. A. Gauthreaux, H. T. Coss, and M. Hare for their help. This work was supported by National Science Foundation grants to the Highland Biological Station, and a Clemson University FRC grant.

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¹ Howard, R. R., and Brodie, jun., E. D., *Amer. Zool.*, **10**, 475 (1970).

² Brower, J. V. Z., *Evolution*, **12**, 32 (1958).

Standing with a Bent Knee

I AM currently investigating knee forces by techniques similar to those already successfully used on the hip joint¹. Although this work is not yet complete, it is difficult to reconcile Dr Roberts' article² with my observations and I should be glad of the opportunity to comment on this from an engineering viewpoint.

From the lever factors y/x in Fig. 3 of the article, the reader is given to understand that the equilibrium of the knee is produced by the force in vastus crureus (of the quadriceps group) acting directly about the joint centre. But this force acts on the patella which functions rather like a pulley attached to the femur and the equilibrium of the knee in the position shown must be due to the force in the patellar ligament. While the forces are similar in magnitude, they have different lever arms and the patellar ligament lever arm should surely replace y in the figure.

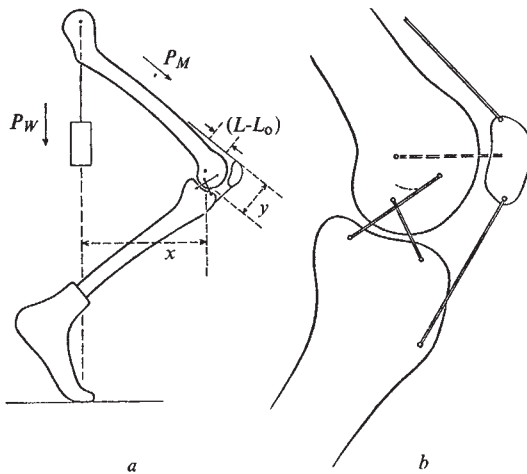


Fig. 1 a, Diagram of model leg to show how the offsets x and y were measured. b, Detail of the pivot points at the knee. The dotted line shows the bone profile round which the posterior ligament becomes wrapped in full flexion of the knee.

The model configuration shown in Fig. 2 (shown here as Fig. 1) is a most unusual position to adopt when standing on one leg. The superimposed body weight is shown acting through the hip so that the trunk must be approximately erect to bring the centre of gravity above the hip joint; the knee is flexed at right angles while standing on the tips of the toes. I have tried to assume this posture and invited several of my colleagues to do so with almost complete lack of success except among the young and athletic who, if they could do it, found the position most uncomfortable. The natural tendency when trying to assume this position on one leg is to incline the trunk forward, thereby moving the line of gravity toward the knee to reduce the force required from the knee extensors. A good example of this forward inclination of the trunk occurs in the common and similar activity of rising from a chair without assistance from the arms. Ciné film records, taken in the laboratories here, show that in extreme cases subjects bring their trunk practically horizontal when rising in this way.

Observation confirms that one-legged support is normally confined to smaller angles of knee flexion than that depicted. Yet, at very small angles of knee flexion, the line of gravity passes behind the joint, bringing the flexors into action and causing the conditions assumed by Dr Roberts to break down. It follows that the simple spring action postulated can only normally occur over quite a small range of knee angles. A further assumption implicit in the theory is that the centre of gravity will only move vertically but, in all normal activities,

including balancing, horizontal movements also occur, since the centre of gravity is always free to move over a region equal to the support area of the foot. Such movements, which are likely to be as frequent as vertical movements, would require immediate compensation by the knee muscles, but would not of course affect the length of quadriceps so that spring action could be of no assistance.

In an activity such as walking, it has been known for many years that complex changes in the moment of the resultant force about the knee occur all the time³. The prop and paddle concept and the linear spring concept are surely an oversimplification of the real situation.

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¹ McLeish, R. D., and Charnley, J., *J. Biomech.*, 3, 191, (1970).

² Roberts, J. D. M., *Nature*, 230, 499 (1971).

³ Bressler, B., and Frankel, J. P., *Trans. Amer. Soc. Mech. Eng.*, 72, 27 (1950).

DR ROBERTS writes: In considering the lever factor, Dr McLeish looks at the force exerted by the muscle on the patella. I am looking at the forces on the femur. We are both neglecting the turning moment exerted by the compressive force between the patella and the end of the femur because the line of action of this force is offset by only a small distance from the axis of the joint. It is this force, however, that accounts for the difference between the actions of the muscle force above and below the patella.

The limb posture in Fig. 2 (shown here as Fig. 1) is less unusual than Dr McLeish maintains. The trunk does not have to be upright, if it is balanced by the free leg, neither does the line of the compressive force really have to be vertical, although in a model where a known force is to be applied at a point whose position is indeterminate it is convenient to use a hanging weight and thus to employ the vertical force of gravity.

Dr McLeish treats my article as though I had maintained that the resistance to compression is the only function to be served by the muscles acting at the knee. He thus takes no account of my treatment of the contribution of the two-joint muscles. Their action in exerting a turning moment at the hip is sufficient to account for all the situations that Dr McLeish adduces in refutation. My thesis, that it may be profitable for purposes of analysis to examine the prop and paddle actions separately, is not affected by his argument.

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Mechanism of Absorption of Phosphate from Soil by *Endogone Mycorrhizas*

ECTOTROPHIC mycorrhizas have often been shown to benefit the growth of their host plants, and similar results have also been found for endotrophic mycorrhizas formed by *Endogone* species^{1,2} which are widespread on agricultural crops and are of considerable importance. Several workers have concluded

that these growth responses are caused by an increased uptake of phosphate from the soils and possible mechanisms have been suggested² to account for this increased uptake. The purpose of our work was to distinguish between some of these possibilities, and we report preliminary results here.

A sandy loam soil of low phosphate status was labelled with ³²P and partially sterilized by γ -irradiation to eliminate indigenous fungi. Onions were grown in the soil in pots and half of these plants were inoculated with *Endogone mosseae* (yellow vacuolate spore type) by the method of Hayman and Mosse⁴. Onions were harvested on four occasions during a period of 4.5 weeks; at each time we measured dry and fresh weights and phosphorus (P) contents of root and shoot, count rate of plant tissue, total root length and the phosphate concentration in the soil solution. Infected plants were clearly larger after 2 weeks and had five times greater dry weight, and two times greater P percentage, than control plants at the final harvest. Infected plants were shown by microscopic examination to have endotrophic mycorrhizas and the hyphae extended into the soil around their roots.

The specific activities of P in the plants (corrected for P in the seed), were almost identical for mycorrhizal plants and controls at all harvests (Table 1). The specific activity of P in equilibrium with the soil solution was measured before, during and immediately after the end of the experiment. The mean of the three determinations, 35 c.p.m., is so close to the mean for the specific activity of P in the plant that it is likely that both groups of plants absorbed their phosphate from the soil solution or forms in rapid equilibrium with it. Neither roots nor hyphae seem therefore to solubilize non-labile inorganic phosphates or hydrolyse organic phosphates.

It is well known that phosphate ions have small diffusion coefficients in soils, and that there is likely to be a serious depletion of phosphate in solution immediately around absorbing roots^{5,6}. The more actively the roots absorb, the greater this depletion will be. A root cannot, however, reduce the concentration of ions in solution at its surface below zero, and the rate of diffusion to such a cylindrical "zero sink" in the soil thus sets an upper limit to the uptake rate of a simple root. No process which occurs solely within the root can affect this.

it is uncertain whether the soil properties, such as water content and bulk density, are the same there as in the whole soil. Our estimate of the maximum inflow (uptake per unit length of root per second) is thus approximate, but is unlikely to be in error by more than a factor of 2. Using the simple model of Brewster and Tinker⁷ with an average phosphate concentration in the soil solution of 5×10^{-6} M, an estimated mean diffusion coefficient of 3×10^{-9} cm² s⁻¹ and an impedance factor of 0.3, we calculate it to be 3×10^{-14} mol/cm s⁻¹. The measured mean inflow to the control plant roots (Table 2) calculated by the formula in ref. 7, could thus be accounted for if all roots were active, but it is highly unlikely that the inflow to mycorrhizal roots could be transported by diffusion alone (Table 2). Because mass flow in the soil solution transported less than 1% of the absorbed phosphate, we conclude that entry into and subsequent transport through mycorrhizal hyphae occurred. Mechanisms which postulate a more active root cylinder cannot account for the results, because all the control plant roots were already functioning at near maximum efficiency. The hyphae may, of course, always produce extra absorbing surface, but the distribution of this surface with respect to the root is crucial—if it is immediately adjacent to the root surface, it merely competes with this for the diffusion-limited supply, but if it is dispersed away from the root, it is outside the root depletion zone, and its uptake is a net gain to the plant. This distinction may be particularly important in similar work on ectotrophic mycorrhizas, where the fungal sheath lies on the root surface.

These results thus support the simplest theory of the mycorrhizal effect, namely, that the hyphae form a better distributed surface for absorbing P from the soil solution than the root alone. If our results are confirmed with other crops and soils, they may be of value in deciding where *Endogone* infection can improve phosphate nutrition in crops. It is implicit in our explanation that this improvement should occur on soils where the maximum P inflow to a "zero sink" root is less than that required by the plant for maximum growth rate.

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- ¹ Gerdemann, J. W., *Ann. Rev. Phytopathol.*, **6**, 397 (1968).
- ² Harley, J. L., *The Biology of Mycorrhiza* (Hill, London, 1969).
- ³ Daft, M. J., and Nicolson, T. H., *New Phytol.*, **65**, 343 (1966).
- ⁴ Hayman, D. S., and Mosse, B., *New Phytol.*, **70**, 19 (1971).
- ⁵ Lewis, D. G., and Quirk, J. P., *Plant Soil*, **26**, 445 (1967).
- ⁶ Drew, M. C., and Nye, P. H., *Plant Soil*, **33**, 545 (1970).
- ⁷ Brewster, J. L., and Tinker, P. B., *Proc. Soil Sci. Soc. Amer.*, **34**, 421 (1970).

Table 1 Specific Activity of Phosphate absorbed by Infected and Uninfected Onion Plants, Measured at Four Harvests

Plant age (days)	Infected plants c.p.m. $\times 10^8$ /mol P	Uninfected plants c.p.m. $\times 10^8$ /mol P
23	44	44
37	38	39
44	36	36
54	35	35

Table 2 Inflow of Phosphorus into Mycorrhizal (M) and Control (C) Onion Roots during Three Successive Growth Periods

Growth period	Duration (days)	Inflow mol/cm root/s $\times 10^{-14}$	
		M	C
Harvest 1-2	14	17	5
Harvest 2-3	7	22	1.6
Harvest 3-4	10	13	4.2

The calculation of this maximum possible rate of uptake into a root system in a soil with defined properties is possible, but it is particularly difficult for phosphate. First, the diffusion coefficient for phosphate in the soil depends on concentration so that an average value has to be used⁶. Second, most phosphate diffusion occurs within a very short distance of the root, and

Rapid Leaf Closure of *Mimosa* in Response to Light

VARIOUS higher plants make rapid movements in response to stimuli¹⁻³, among them *Mimosa* which rapidly closes its leaves when touched. We wish to report a series of experiments demonstrating such fast closure in response to light alone. Usually, light causes opening of the leaves, although it is

known that after a few hours in the dark, light (<550 nm) will cause the petiole to fall rapidly^{4,5}.

Mimosa pudica was grown in soil from seed, raised in a greenhouse with a cycle of 16 h light and 8 h dark under natural and fluorescent lighting. When the plants were 3 months old, they were changed to a cycle of 14 h light and 10 h dark under incandescent lighting. Experiments were run when the plants were 4–9 months old. From each plant, leaves (with at least ten pinnule or leaflet pairs per leaf) were excised and floated in distilled water. This treatment produced results no different than those obtained for the entire plant. The leaves were illuminated for 30 min (to open the leaflets fully) with an incandescent (white) light source (45 mW/cm²/s) 35 cm from them. A water filter was used to absorb heat and in no case did the temperature of the air around the leaflets increase more than 0.1° C. Experiments were begun at least 3 h into, and finished more than 2 h before the end of, the photoperiod. After the 30 min opening period, a variety of light–dark presentations was used in an attempt to elicit a fast closing response of the leaflets. For each cycle of light and dark, twelve plants were chosen randomly as to the order they were to be run in each cycle. Two different leaves from each plant were used for each cycle tested. After the initial period of illumination the leaves were given dark periods from 1 to 20 min followed by a 10 min illumination from the same white light source. The treatments used and the results are presented in Table 1. A dark period of at least 5 min followed by light is necessary to produce rapid leaf closure, defined as movement of at least two of the pinnule pairs, initiated and completed within 3 s. The latency of closure is the time elapsed before the rapid movement begins. By Wilcoxon tests, there is no difference in latency among the D20, D10 and D7 treatments ($P > 0.05$, two tailed), but the D5 and D6 treatments have a significantly longer latency ($P < 0.01$, two tailed). This suggests that the response is becoming weaker as the dark period approaches D5; below this point, there seems to be no gradual increase in latency, but rather a sharp cutoff in the response. Because of variability in the sensitivity of leaflets, as Fondeville *et al.*⁵ noticed, the exact point between closing and remaining open between D4 and D5 is difficult to determine. Those treatments that produced rapid closure could be repeated at least three times with rapid closure continuing and with no significant change in latency on the third presentation. But, when switched to D1, the leaves remained open during the light. Similarly, the D1 treatment followed by light would not produce closure even when repeated three times, but when switched to D10, rapid closure occurred during the light period.

Table 1 Response to White Light

Treatment (min) with dark (D)	Response (latency in s) to light of 10 min	
	First presentation	Third presentation
D20	Close (61)	Close (70)
D10	Close (54)	Close (49)
D7	Close (52)	Close (59)
D6	Close (79)	Close (77)
D5	Close (94)	Close (90)
D1, D2, D3, D4	Open	Open

Twenty-four leaves were used for each treatment.

To determine what wavelengths of light might be effective in producing this behaviour, interference filters were used with coloured filters and an incandescent light source to produce sharp peaks of light with known spectral characteristics. Two leaves from each of twelve plants were used again exactly as in the above experiment but only with a D10 treatment followed by light of 10 min duration. At an intensity of 0.3 mW/cm²/s,

peak wavelengths of from 420–900 nm produced no closure. As the intensity (or energy level) was raised gradually to catch the peaks of the action spectrum, only certain wavelengths produced closure. At an intensity of 0.4 mW, peaks at 460–520 and 620–660 were the only wavelengths producing closure. Raising or lowering the intensity did not discriminate any further among these wavelengths.

We cannot say how these treatments produce rapid closure, which presumably involves changes in the permeability of the cell membrane of the motor cells in the pulvini with a subsequent change in turgor pressure³. It has been suggested, however, that pigments, phytochrome in particular, act, either directly by being bound to membranes or indirectly through some mediating substance, to control permeability change⁶.

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- ¹ Lloyd, F. E., *The Carnivorous Plants* (Ronald Press, New York, 1942).
- ² Ball, N. G., in *Physiology of Plant Growth and Development* (edit. by Wilkins, M.) (McGraw-Hill, New York, 1969).
- ³ Sibaoka, T., *Ann. Rev. Plant Physiol.*, **20**, 165 (1969).
- ⁴ Fondeville, J. C., Borthwick, H. A., and Hendricks, S. B., *Planta*, **69**, 357 (1966).
- ⁵ Fondeville, J. C., Schneider, M. J., Borthwick, H. A., and Hendricks, S. B., *Planta*, **75**, 228 (1967).
- ⁶ Galston, A., and Satter, R., *Rec. Adv. Phytochem.*, **4** (in the press).

A New Neandertal Child Mandible from an Upper Pleistocene Site in Southern Italy

A NEW mandible of a Neanderthal child was found by chance by Mr Adolfo Berdar on September 6, 1970, during the opening of a motorway at the Upper Pleistocene site at Archi, near Reggio Calabria, in Italy.

The geology of the site can be summarized as follows. The hill south-west of the town of Archi (Fig. 1), a remnant of one of the lowest terraces of the Aspromonte and cut by the valley of the River Torbido, consists of a sandy gravel series of Upper Pleistocene age. This terrace contains four complexes—*A*, *B*, *C* and *D* (see Fig. 2, I). Grey clays containing Pteropoda, *Dentalium*, *Nucula* and cyathoid corals from the higher Pliocene (*A*) are overlapped by a clinostratified sandy-gravel formation (*B*). The *B* formation which dips gently is younger than Calabrian and is probably Sicilian in age. It has abundant neritic malacological fauna (*Pectunculus*, *Modiola* cfr. *modiolus*, *Arctica islandica*, *Lucina glacialis*, *Cardium norvegicum*, and the like). A sharp unconformity separates this group from the overhanging fluvio-deltaic gravels and sands, the *C* series. Three subdivisions of *C* can be distinguished. First, horizontally layered coarser gravels from crystalline and metamorphic rocks (*C*-1), with iron oxidation on the cemented top. Second, "grey sands" (*C*-2), which consists of gently inclining lenses of sand and sandy gravels, some with ochraceous oxidation, with scattered marine shells (for example, *Spondylus gaederopus* and very rounded *Pectunculus* fragments, *Patella ferruginea*, *Anomya*, *Mytilus*). These may be remnants of reworked shells of the subjacent Sicilian, and of a destroyed Tyrrhenian (that is to say, *Strombus*) beach. In the higher

levels there are frequent pumice fragments (*V*), which are evidence of volcanic activity in the Northern Sicilian area at that time. Third, after a crossed-bedding layer of fluvial origin, there is a layer of gravels and sand (*C-3*) 3–4 m deep, in which the human mandible was collected (*M*). Various other embedded bones of mammalian fauna were also found—*Paleoloxodon antiquus-italicus* of great size (for example, molar teeth, femur, vertebrae), *Dicerorhinus merckii*, *Bos* cfr. *primigenius*, *Cervus elaphus* of large size and *Megaceros* sp. Among the very scarce avifauna, a proximal extremity of tibia of the great auk, *Alca impennis* Linn. was found. Fossils of the great auk have also been discovered in the Mediterranean area at the Romanelli cave (Otranto)^{1,2} and at Devil's Tower (Gibraltar)³.

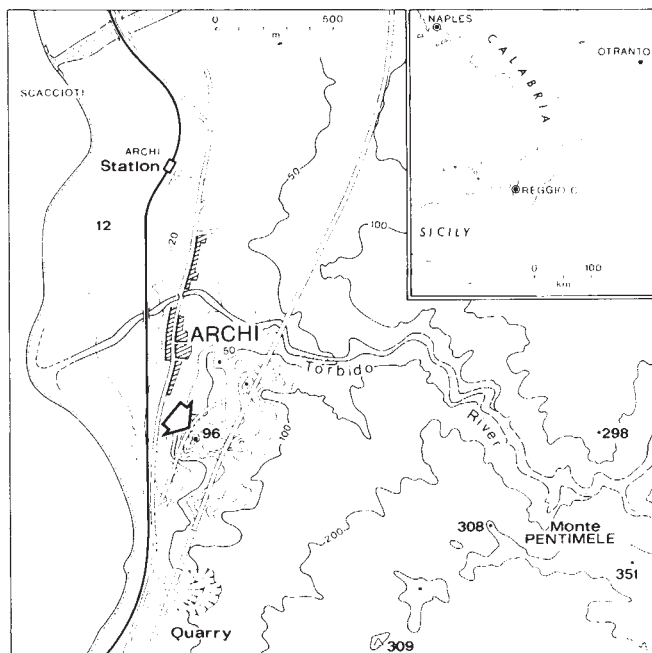


Fig. 1 The site near Reggio Calabria (height in m).

The Archi series continues with a very thin silty layer (*S*) containing freshwater and terrestrial molluscs (*Limnaea*, *Paludina*, *Bythinia*, *Anodonta* and *Buliminus*), and a sandstone with silicified wood and other badly conserved remnants of fossil flora. Next, there is a conglomerate which contains brackish oligohaline facies fauna (*P*), rich in *Tapes decussata* and *Cardium edule lamarki*, together with *Cardium papillosum*, *Solen*, *Venus* and Ostracoda (*Cyprideis*, *Xestoleberis*). The presence of this fauna suggests that there was a marine transgression which was followed by a rise of about 92 m in this part of the coast, so creating the terrace. The series ends with marls which contain traces of flora and gravels (*D-1*). An unconformity separates *D-1* from the topmost "red gravel formation" (*D-2*), which also contains *Paleoloxodon antiquus-italicus*. A sizable cut and a deepening of the valleys resulted therefore from this considerable neotectonic rise, while an opposite movement of the same size took place on the side facing the sea.

This analysis of the Archi series has a bearing on the Pleistocene history of the Messina Strait area. A more recent origin must now be attributed to layers such as *C*, which have previously been regarded as probably Sicilian or even older^{4–6}. Many radial valleys in the Aspromonte must now be considered to be very young. A considerable isostatic rise in the land must have occurred in the late Pleistocene with continuation in the Holocene and this must have measured more than 100 m in Calabria and north-east Sicily. As observed in Sicily⁷, neotectonic movements in Southern Calabria were not uniform or

contemporary. It seems that there were two important general changes in the palaeogeography of this region—between the Sicilian (marine, *B*) and the late Pleistocene (fluvial, deltaic and brackish facies, *C*), and between (*C*) and the uppermost continental (*D*).

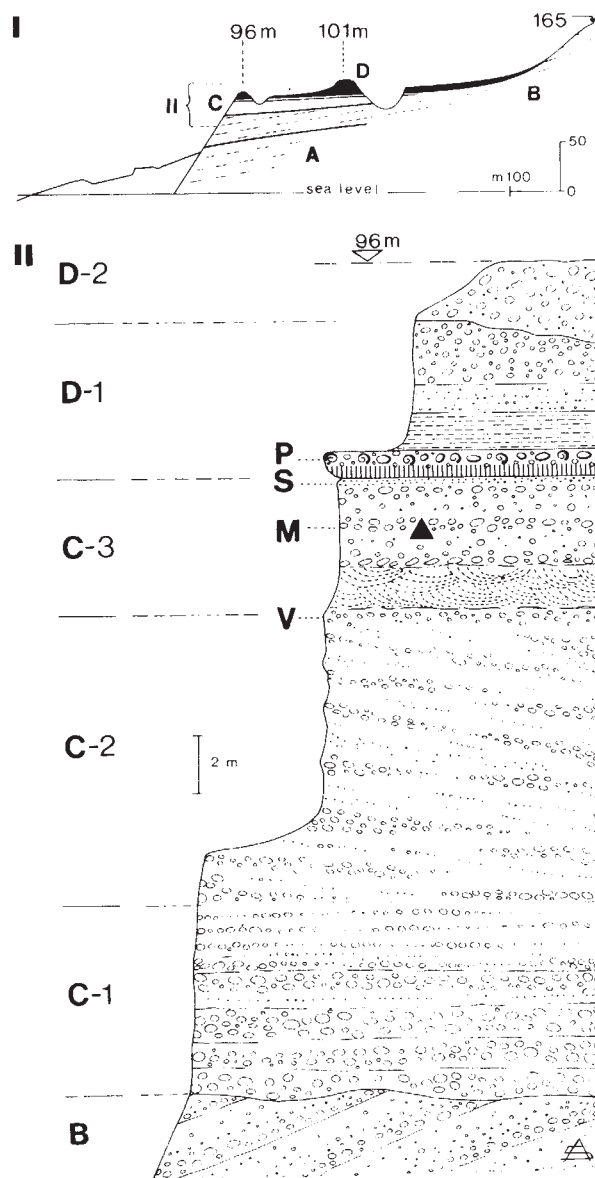


Fig. 2 Stratigraphy of the Archi Hill deposits—the 90–100 m terrace. I, Generalized section; II, the Upper Pleistocene series. Explanation in text.

The Neanderthal mandible reached our laboratories embedded in a hard concretion deposited by lime-impregnated water. When fully cleaned it was seen to be a highly mineralized and an incomplete child's mandible (Fig. 3). Both the ascending rami had been broken away long since by a fracture behind the alveolus containing the crown of the unerupted first permanent molar. All the deciduous incisors and the canine of the right side are missing, but the other deciduous teeth—the canine on the left side and all the molars—are in position. X-ray examination revealed the presence of the germs of all the permanent teeth from *I*₁ to *M*₁ which suggests that the mandible belonged to a child aged between 5 and 6.

The mandible is markedly massive and there is little doubt that it is male. The front of the jaw contains some interesting

features (Fig. 3b). The chin definitely recedes, that is, the alveolar border projects and the basal margin falls back. But the slightly inward curve of the outline which corresponds to the alveolar part suggests an initial incurvatio mandibulae anterior. In the basal margin a low triangular elevation in the median line, the trigonum mentale, is visible and can be felt when the finger is passed over the bone. At the lower margin of the symphysis the trigonum continues into the interdigastric spine, so that it has a rhombic shape.

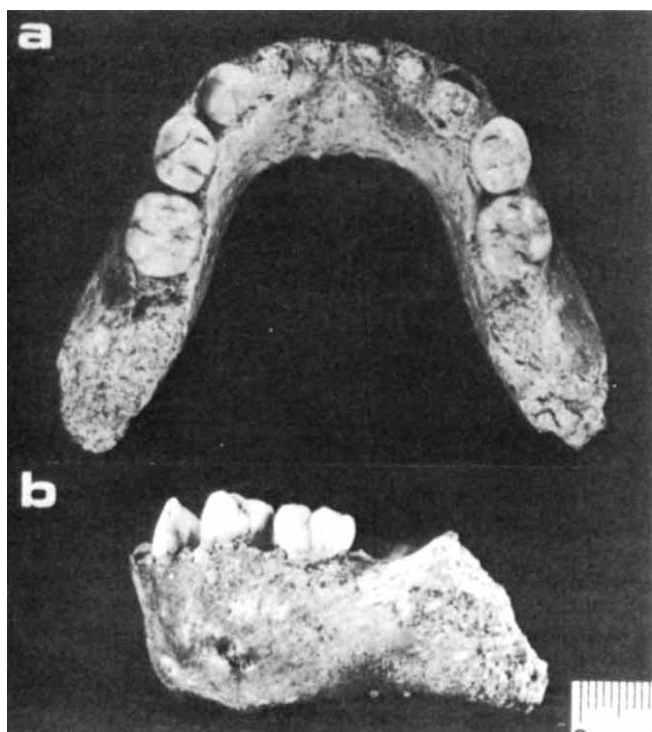


Fig. 3 a, Profile view (left side); b, occlusal view in alveolar plane orientation of the mandible.

The inner surface of the frontal part immediately below the alveolar border rises to a flat, smooth, diffuse swelling, the torus superior. Below this and without any break the muscular area is very clearly developed. It consists of a faint rough surface with two spine-like formations, one on each side. The spine on the left is long and narrow, whereas that on the right is very small. In the mid-line at the boundary between the torus superior and the muscular area there is one foramen supraspinosum.

The outline of both the inner and outer surface of the symphysis is given in Fig. 4a. The drawing was obtained using Sergi's diagraph and by making a cast of the jaw and cutting it at the required point. Measurements of the angle of symphysis (according to Weidenreich⁸), the angle of the chin (see McCown and Keith⁹) and the height and the index of the position of the foramen supraspinosum were taken from the drawing (Table 1).

On the lateral surface of the body of the mandible the foramen mentale is found opposite the first molar, about 10.2 mm to the right and 11.8 mm to the left of the alveolar border, and about 5.6 mm to the right and 6 mm to the left of the lower margin. The opening is of a long oval form, with an incipient septum between the front and back parts. The prominentia lateralis is well developed but, as would be expected in a child of between 5 and 6, it is found in the posterior alveolar part, that is to say, on the labial side of the place to be occupied by M₁.

On the inner surface of the body of the mandible there is no fossa sublingualis as a separate anterior part of the fossa

subalveolaris beyond the linea mylohyoidea. The fossa subalveolaris itself ends at its lower boundary in an appreciable elevation which strengthens the inner surface of the lower margin of the jaw.

A drawing was made of the alveolar arch—the horseshoe-shaped area occupied by the alveoli—with the labial and lingual limits of the alveoli taken as boundaries. From this drawing the length and breadth of the arch and the mandibular index were measured (Table 1).

Table 1 Measurements of the Mandible (Linear Measurements in mm)

Height of the symphysis	22	
Height of the foramen supraspinosum	9	
Height of the body	20.7 (R)	20.9 (L)
Height of the body at M ₂	18.4 (R)	19.2 (L)
Thickness of the body	11.8 (R)	12 (L)
Thickness of the body at M ₂	13.4 (R)	13.7 (L)
Bimental width	40.6	
Width between buccal margins of M ₂	50.1	
Width between lingual margins of M ₂	34	
Length of the anterior alveolar arch	27.8	
Breadth of the anterior alveolar arch	44	
Index of robustness	57 (R)	57.4 (L)
Index of the foramen supraspinosum	40.9	
Index of the anterior alveolar arch	63.2	
Angle of the symphysis	65°.5	
Angle of the chin	81°	

R, right side; L, left side.

The crowns of the teeth are well preserved, and the cusps are only slightly abraded. The measurements are given in Table 2. The left canine has a strongly convex buccal surface bordered by a slight basal arch-like swelling which rises on each side. This basal structure can be considered a typical cingulum. The lingual surface is slightly concave and is bordered on all three sides by a thickened rim. The mesial side looks like that of an incisor. In the first milk molar the trigonid which has three cusps and both mesial and distal crests is markedly higher than the talonid. The talonid is an oval basin separated from the trigonid by the distal trigonid crest and the slope of the metaconid. The buccal surface is bordered at its base by a pronounced cingulum, but no definite tuberculum molare can be seen. In the second milk molar the trigonid is not higher than the talonid and is not retained in its entirety. In particular there is no longer any direct connexion between the protoconid and metaconid. The talonid is large and has some fine furrows in it which indicate the boundaries of the cusps and the presence of some wrinkles on the lingual side. X-rays of the teeth revealed a marked degree of taurodontism in the molars.

Table 2 Measurements (in mm) of the Crown of the Teeth

	C (L)	M ₁ (R)	M ₂ (R)	M ₁ (L)	M ₂ (L)
Mesio-distal length	7.2	9	10.6	9	10.4
Bucco-lingual breadth	6.6	7.7	9.3	7.4	9.2
Height	8.2	5.8	5.6	6	5.8
Robustness of the crowns	47.5	69.3	98.6	66.6	95.7

R, right side; L, left side.

A survey of all the anatomical features leads to the conclusion that this mandible possesses the primitive features found in Neandertals. A reliable comparison with other Neandertal specimens is difficult because so few are available,

especially in this age group—there are only the mandibles of the Devil's Tower (Gibraltar) child³ and of Skhül I and Skhül X⁹.

The resemblance of this mandible with that of the Devil's Tower child is very striking in many respects—the angle of the symphysis (68°), the height of the symphysis (21 mm), the height of the body at M₂ (19 mm), the width between the buccal margins of M₂ (54 mm), the width between the lingual margins of M₂ (33 mm), and the length (27.5 mm), the breadth (46 mm) and the index of the anterior alveolar arch (59.8). The angle of the symphysis reported here for the Devil's Tower mandible was measured using the median section through the symphysis, according to Garrod *et al.*³ (Fig. 4b). Moreover, it was assumed that this section was correctly oriented to the alveolar plane, and the position of the incision was deduced from the height of the symphysis (21 mm). The value obtained in this way does not correspond to that obtained by Weidenreich⁸ (75.5°), who used a drawing of the section quite different from the original one. Finally, the angle of symphysis in our mandible is very close to that of the child of Ehringsdorf¹⁰ (65°) according to Weidenreich⁸.

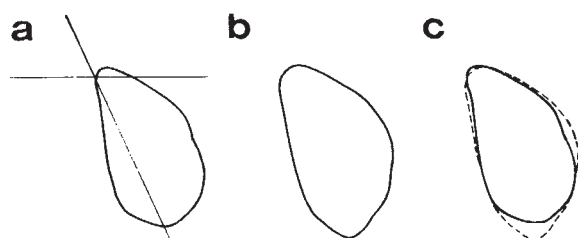


Fig. 4 Drawings in natural size of the section through the symphysis mentis in the mandible of Archi (a) and in that of Devil's Tower (b). Superimposition of the two drawings for comparison (c).

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- ¹ Blanc, G. A., *Arch. Antropol. Etnol.*, **50**, 1 (1920).
- ² Blanc, G. A., *Arch. Antropol. Etnol.*, **58**, 155, 365 (1928).
- ³ Garrod, D. A. E., Dudley Buxton, L. H., Elliot Smith, G., and Bate, D. M. A., *J. Roy. Anthrop. Inst.*, **58**, 33 (1928).
- ⁴ Gignoux, M., *Ann. Univ. Lyon*, NS, **36**, 77 (1913).
- ⁵ Mantovani, P., *Boll. R. Com. Geol. Ital.*, **9**, 443 (1878).
- ⁶ Malatesta, A., Motta, S., and Iacobacci, A., *Piano di Studi Sullo Stretto di Messina, I: Ricerche Geologiche* (Regione Siciliana, Palermo, 1961).
- ⁷ Trevisan, L., *Geol. Rdsch*, **43**, 207 (1955).
- ⁸ Weidenreich, F., *Palaeont. Sin.*, Ser. D, fasc. 3, 7, 1 (1936).
- ⁹ McCown, T. D., and Keith, A., *The Stone Age of Mount Carmel: The Fossil Human Remains from the Levallois-mousterian, 2* (Clarendon Press, Oxford, 1939).
- ¹⁰ Virchow, H., *Die menschlichen Skelettreste aus dem Kämpferschen Bruch im Travertin von Ehringsdorf bei Weimar* (Fischer, Jena, 1920).

Submillimetre Wave Sensing of Nocturnal Moths

THE development of insects resistant to chemical pesticides has created a strong need for optical traps to control populations of economically important insects. But first it is necessary to understand their response to electromagnetic radiation. In 1968 preliminary tests indicated that pulsed coherent radiation of 337 μm wavelength might be attractive to insects. We have now made a behavioural and theoretical study of this phenomenon, using the fall army worm (*Spodoptera frugiperda*), the corn earworm (*Heliothis zea*), the Indian meal moth (*Plodia interpunctella*) and the codling moth (*Carpocapsa pomonella*).

Behavioural experiments were conducted using hydrogen cyanide and water vapour lasers providing signal at wavelengths of 28, 118, 311 and 337 μm and power levels between 16 and 50 mW to evaluate the frequency dependence of response. We used male *S. frugiperda* and *H. zea*, between 12 and 72 h old. All the tests were conducted during the period of peak nocturnal activity (8.00 p.m. to 1.00 a.m., EST). The experimental setup is illustrated in Fig. 1; the observation procedure has been described already¹. The test chamber was a plastic cylinder of 4.5 inches in diameter and 80 inches long and most moths were at the end opposite the laser before the power was turned on. The number of moths attracted to the laser was counted as a function of time after the laser was switched on.

Precautions were taken to insure that the moths responded to the submillimetre wave radiation and not to any other stimulant. Experiments were conducted in a darkened room (21° C) and moths were counted using a red filtered flashlight. To reduce the visible and ultraviolet radiation emitted from the lasers, the ends of the test chamber were covered with black polyethylene with zero transmission in this region of the spectrum. The test cage was also covered with a black cloth to screen it from the visible radiation emitted from the laser plasma tube. To eliminate any doubt with regard to the test room bias the laser was detuned for 10 min and behaviour was observed before each experiment. (When the laser is detuned, the output power drops to zero while the visible and ultraviolet radiation remain at the same level.)

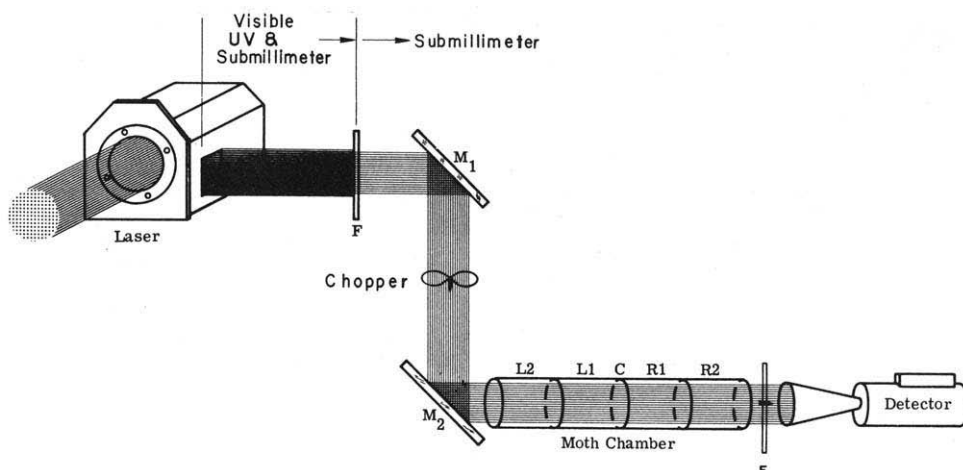
A strong attraction of both species to 337 and 311 μm radiation was observed, but, the response at 311 μm was less pronounced than at 337 μm (Fig. 2). *S. frugiperda* was not attracted to 28 or 118 μm radiation. The response of male *S. frugiperda* to an attractant light (7.5 W BL lamp) in similar environmental conditions is included for comparison. Chopping the radiation at a rate between 600 and 3,000 min^{-1} did not affect the response at 337 μm , but the attraction of *H. zea* to 118 μm increased slightly with a chopping rate of 2,400 min^{-1} . Further experiments indicate that the 337 μm radiation induces changes in the activity of the moths and reduces mating potential. Exposure of both species to 337 μm (50 mW) for 0.5 h inactivated them for the rest of the night although the experiment was conducted at the beginning of nocturnal activity period. The effect was more noticeable with *S. frugiperda*. Also, when fifteen virgin female *H. zea* were introduced in a cage containing

Table 1 Physical Dimensions and Propagation Characteristics of Nocturnal Moths

Moth species	D _{max} (μm)	D _{min} (μm)	D _{av} (μm)	R	λ_1 (μm)	λ_{op} (μm)
<i>H. zea</i>	216	109	167	0.705	266	328
<i>S. frugiperda</i>	210	116	160	0.8	220	347
<i>C. pomonella</i>	163	137	150	0.73	190	380
<i>R. interpunctella</i>	110	75	94	0.71	130	224

D_{max}, maximum outside diameter; D_{min}, minimum outside diameter; D_{av}, average diameter; R, ratio of inside to outside diameter; λ_1 , cut-off wavelength of H₀₁ mode; λ_{op} , optimum operating wavelength.

Fig. 1 Diagram showing laser and moth test chamber. M_1 and M_2 are mirrors and F is a polyethylene filter.



twenty-nine males, one each 5 min (10.30 p.m. to 12.30 a.m., EST) in the presence of 16 mW at 337 μm wavelength, fourteen out of them were attracted to the laser and no mating was observed. These effects may be similar to respiration changes induced by a BL attractant lamp².

Looking for a theoretical basis for these responses, we conducted experiments which show the failure of incoherent infrared sources³, and coherent radiation from Klystrons (0.8 to 3.5 cm) and a Gallium Arsenide diode (0.85 μm) to elicit the response of the moths. This indicates the absence of electromagnetic thermal detectors in the moth system and suggests that moths respond to invisible radiation through tuned structures. Macroscopically the main spines of these moths' antennae are hollow thin walled tubes with tapered cross section and periodic variation in the outside diameter. This structure is suitable as a dielectric antenna coupling electromagnetic radiation between the moth and its environment.

An antenna would function properly if the radiation pattern was highly directive and the dielectric tube able to propagate electromagnetic waves over the useful bandwidth. Since pure E or H modes can be excited in dielectric tubes only in the case of perfect circular symmetry, it is reasonable to assume that only hybrid modes exist on moth antennae. Since all the modes except the HE_{11} mode are characterized by a cut-off wavelength⁴ and the radiation pattern is highly directive when only one mode is excited; therefore dielectric tube antennae perform properly over the frequency band where only the HE_{11} mode can be propagated. (The subscript numbers indicate the number of half wavelengths which the interference pattern of the mode possesses in the plane of the tube cross section.)

The average of the pertinent physical dimensions of several moth antennae, together with their propagation characteristics, using a value of 2.5 for the relative dielectric constant⁵, are listed in Table 1. The optimum wavelength λ_{op} is based on the criteria by Thorell⁶, which compromise between antenna gain and directivity. Measured values of D_{min} were used to calculate the cut-off wavelength of H_{01} modes (lowest order cut-off mode). The previous discussion shows that electromagnetic radiation of wavelength λ_{op} could be efficiently coupled into the moths. As the wavelength deviates from λ_{op} the ability of the moths to respond to radiation drops gradually.

In conclusion, insects are strongly attracted to a submillimetre radiation source. Theoretical study on the moth antennae together with the frequency dependence of their response strongly suggests that the antennae may be the receptor through which insects are stimulated to respond. The response is very pronounced and has strong potential for optical traps to control the population of pest insects.

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¹ Levengood, W. C., *Mich. Academician*, **1**, 31 (1969).

² Levengood, W. C., *Z. Verh. Physiol.*, **62**, 153 (1969).

³ Levengood, W. C., and Eldumiati, I. I., *US Dept. Agric. Rep.* (1970).

⁴ Kiely, D. G., *Dielectric Aerials*, 92 (Wiley, New York, 1953).

⁵ Callahan, P. S., *Appl. Optics*, **7**, No. 8, 1425 (1968).

⁶ Thorell, L., *The Antenna*, 176 (Chapman and Hall, London, 1960).

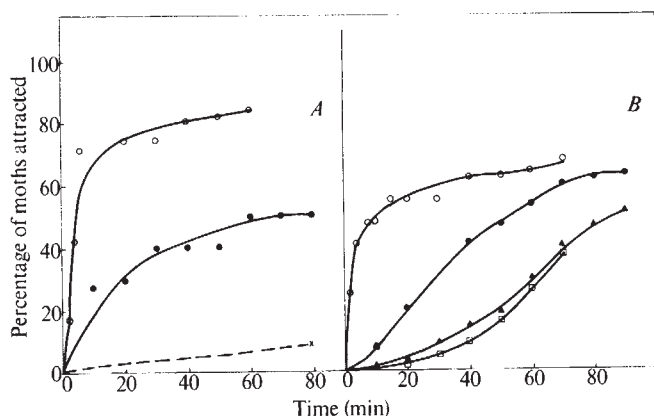


Fig. 2 Response of male *S. frugiperda* (A) and male *H. zea* (B) to electromagnetic radiation. The radiation wavelengths are 337 μm (○), 311 μm (●), 118 μm (▲) and 28 μm (□). The dashed line is the response to incoherent BL source. *S. frugiperda* did not respond at 118 and 28 μm .

Lymphocyte Sensitization to Basic Protein of Brain in Malignant Neoplasia: Experiments with Serotonin and Related Compounds

It has been found that blood lymphocytes from patients suffering from malignant neoplasia (even in an early stage) are sensitized to a basic protein¹ (encephalitogenic factor, EF) derived from human brain². They react even more markedly (but to a strikingly similar degree) with a basic protein derived

from a variety of human tumours³. During a visit to Dr Carnegie's laboratory some months ago one of us (E. J. F.) was shown the complementary structure of the serotonin molecule and the antigenic determinant (since published⁴) and realized that since the electrophoretic method¹ depends on interaction of EF with sensitized lymphocytes, structures complementary to the EF determinant might be able to mask it and so block the reaction.

In EF from human brain the main encephalitogenic determinant is associated with the sole tryptophan residue and the equivalent synthetic (eleven amino-acid) peptide⁵ is known to be a potent inducer of experimental auto-immune encephalomyelitis^{5,6}. Carnegie⁴ further suggested that the encephalitogenic determinant might function as a receptor site for serotonin (5-hydroxytryptamine) and so be important in the pathogenesis of both experimental encephalitis and multiple sclerosis.

Here we examine the ability of serotonin and sterically related compounds, as well as other materials with well marked neuropharmacological activity, to block EF-lymphocyte interaction. The "goodness of fit" of these materials to the antigenic determinant has also been assessed.

From patients with cancer (lung, stomach, bowel, breast) 20 ml. of venous blood was removed and lymphocytes separated after preliminary defibrination as described elsewhere^{7,8}. Sensitization of lymphocytes to EF was determined by the cytopherometric method and confirmed previous results. In principle the method depends on the interaction of sensitized lymphocytes with antigen (EF) resulting in the production of a material (dependent on protein synthesis by the lymphocytes⁹) with the property of slowing the electrophoretic migration of normal guinea-pig macrophages. The latter are thus used as a marker system for lymphocyte-antigen interaction. As before, the normal guinea-pig peritoneal exudate was subjected to 100 γ radiation to eliminate the potentiality of the 10–20% admixed guinea-pig lymphocytes to take part in a "mixed reaction" with the added human lymphocytes. Experimental details have been given elsewhere^{7,8}. In brief, the lymphocytes to be tested were incubated together with the irradiated macrophages and antigen for 90 min and mobility of the macrophages measured (t_e). As control the mobility without antigen was estimated (t_c). When the blocking effect of a material was to be examined it was added to the cells and allowed to incubate at room temperature (20° C) for 30 min. The antigen was then added and after 90 min at room temperature the cytopherometric measurements on the macrophages were carried out. In the control experiments antigen was added first and after 1 h incubation the blocking agent added. After a further 30 min measurement of macrophage mobility was made.

Results were expressed as percentage increase in migration time. Thus if t_c = mean migration time of ten cells in each direction of the potential difference (twenty readings) over a given distance under control conditions (that is, lymphocytes + macrophages and no antigen), t_e = mean migration time with antigen added, then $\frac{t_e - t_c}{t_c} \times 100$ was calculated in each case. As in previous work^{1,7,8} all specimens were numbered, scrambled and studied "blind". Results were later decoded.

In general EF produced about 15% reduction in migration rate of macrophages through interaction with lymphocytes from patients with malignant neoplasia¹. A synthetic eleven amino-acid peptide of known encephalitogenic capacity^{5,6} was also found to produce a similar reduction (Table 1). Thus the determinant on EF which reacts with lymphocytes from patients with malignant neoplasia seems to be antigenically similar to the encephalitogenic factor and should (if Carnegie's⁴ speculation is justified) be susceptible to blocking by serotonin. While 10 μ g/ml. of serotonin failed to influence the migration rate, 33 μ g/ml. added before EF resulted in a 10% reduction (as compared with 17.2 when added after EF), and 18.2% when no serotonin at all was present. In later experiments 66 μ g/ml. or 132 μ g/ml. serotonin was used, the former produc-

ing 6.6% slowing and the latter 3.9%. A similar effect was produced by incubating 33 μ g EF with 66 μ g/ml. serotonin for 30 min at room temperature before adding to the cell mixture. In these conditions 3.4% reduction resulted. Because of the small scatter of the individual migration time readings, analysis showed that this was significantly different ($P < 0.001$) from the 6.6% obtained when serotonin was added at the beginning. In all subsequent experiments 132 μ g/ml. serotonin was used and gave a reduction of below 4%, that is in the range obtained in normal (non-sensitized) subjects. Other neuropharmacologically-active or serotonin-related drugs were also tested. These included adrenaline, noradrenaline, acetylcholine, tryptamine, histamine, succinyl choline. None was found to be active in blocking EF-lymphocyte interaction in cancer patients. The protocol of typical experiments is shown in Table 2. It is apparent that serotonin alone has the capacity to block EF. Only D-lysergic acid diethylamide has a capacity to block partially—a result expected on grounds of steric arrangement of the molecule (as would be expected if we were dealing with a steric fit).

Table 1 Blocking Effect of Serotonin; Illustrative Protocols

	A	B	C
EF (a)	17.0	16.3	18.2
Serotonin + EF	3.9 (b)	1.1	10.0 (c); 18.6 (d)
EF + serotonin	17.3	16.4	17.2 (c); 18.6 (d)
	D		E
Synthetic peptide (3.3 μ g/ml.)*	16.3	BP (33 μ g/ml.)	16.1 ($P < 0.001$)
Serotonin + peptide	0	Serotonin + BP	10.4 ($P < 0.001$)
Peptide + serotonin	16.5	BP + serotonin	16.3
	F		G
PPD (33 μ g/ml.)	—	BSA (33 μ g/ml.)	13.6
Serotonin + PPD	14.0	Serotonin + BSA	13.8
PPD + serotonin	17.9 (e)	BSA + serotonin	13.1
	H		
F1 (33 μ g/ml.)	—		
Serotonin + F1	17.4		
F1 + serotonin	18.8		

(a) 33 μ g/ml. unless otherwise stated.

(b) 132 μ g/ml. serotonin—the amount unless otherwise indicated.

(c) 33 μ g/ml. serotonin instead of the normal 132 μ g/ml.

(d) 10 μ g/ml. serotonin instead of the usual 132 μ g/ml.

(e) Both healthy and diseased subjects show PPD values between 15–20%.

Patient	Sex	Age	
A	B. A.	F	45 Carcinoma of the larynx
B	M. L.	F	62 Carcinoma of the breast
C	T. J. L.	M	59 Carcinoma of the bronchus
D	M. P. T.	F	50 Carcinoma of the bronchus
E	C. McA.	M	58 Carcinoma of the antrum
F	J. B.	M	29 Myasthenia gravis
G	W. F. A.	M	47 Sarcoidosis
H	P. A.	F	26 Normal

* Ser-Arg-Phe-Ser-Trp-Gly-Ala-Glu-Gly-Gln-Arg (SF 13a obtained through the courtesy of Dr F. C. Westall⁴).

Having established that serotonin could block the interaction of EF and sensitized lymphocytes in cancer we studied its blocking capacity with other antigens. To do this we took advantage of the fact that all subjects studied both in health and disease have lymphocytes sensitized to the F1 fraction (thyroglobulin) of thyroid gland⁷ and that patients suffering from sarcoidosis are sensitized to a number of antigens including bovine serum albumin¹⁰. Attempts to block F1-lymphocytes inter-reaction by means of serotonin were

Table 2 Blocking of Antigens by Drugs other than Serotonin: Illustrative Protocols

	Patient E	Patient B	Patient D
EF	17.0	16.3	17.4; 16.3 *
Acetylcholine	0.8	—	—
Acetylcholine + EF	16.0	16.1	16.9 *
EF + acetylcholine	16.7	15.0	—
Histamine	0.4	—	—
Histamine + EF	16.3	—	16.3 *
EF + histamine	16.9	—	—
Succinyl choline	0.0	—	—
Succinyl choline + EF	15.1	—	—
EF + succinyl choline	14.6	—	—
Noradrenaline	0.0	—	—
Noradrenaline + EF	18.2	15.7	—
EF + noradrenaline	17.8	15.1	—
Adrenaline	-0.2	—	—
Adrenaline + EF	17.6	—	—
EF + adrenaline	17.3	—	—
Tryptamine + EF	—	15.0	17.2 *
EF + tryptamine	—	14.7	—
D-Lysergic acid diethylamide	—	—	7.8 *

* Peptide (3.3 µg/ml.—see Table 1 note) substituted for EF (33 µg/ml.).

Patients E, B and D are those listed at foot of Table 1. All drugs were at a concentration of 132 µg/ml. except D-lysergic acid diethylamide 50 µg/ml.

unsuccessful. In a typical experiment serotonin (132 µg/ml.) followed by F1 gave 17.4%; and F1 followed by serotonin, 18.8%—results not significantly different. Similarly in a case of sarcoidosis BSA alone gave 13.6%; serotonin (132 µg/ml.) followed by BSA 13.8%; and BSA followed by serotonin 13.1%. It is thus apparent that serotonin has no capacity to block F1 or BSA interaction with lymphocytes. Its blocking effect is specific for the encephalitogenic determinant of EF.

Table 3 Illustrative Protocol of Blocking Experiment with Concanavalin

Patient	Antigen	Concanavalin * + antigen	Antigen + concanavalin
E.A. aged 10 erythema nodosum	EF	16.8	3.8
J.H. aged 57 sarcoidosis ⁹	EF	17.9	—
	BP	16.4	1.3
	Muscle	14.1	2.1
	PPD	18.9	0.9
	Kveim reagent	13.5	1.0
	Ovalbumin	13.6	1.9
	BSA	13.7	1.9
	F1 ⁶ † (thyroglobulin)	18.2	2.0
			17.4

* 30 µg/ml.

† F1 values both in health and disease centre on 20%.

EF, BP, PPD, ovalbumin, bovine serum albumin and thyroglobulin were at a concentration of 33 µg/ml. Kveim reagent was from Public Health Labs, Colindale; 0.1 ml. was used in each test. A supernatant from centrifugation (250g, 10 min) of a 20% suspension of human muscle was prepared and 0.1 ml. used in each test.

Concanavalin was tested for blocking capacity. It was found to block all antigens both in neoplasia and sarcoidosis (Table 3) in the same way as antilymphocytic serum (ALS)¹¹. This blocking activity is thus general and not specific. Both concanavalin and ALS differ from serotonin in that the blocking activity of the latter is limited to EF. It is known that a basic protein which may be extracted from human sciatic nerve (BP) has some antigenic determinant(s) in common with EF (made from human brain)¹²⁻¹⁴. Indeed in patients with malignant

neoplasia (but with no evidence of either central or peripheral nervous system involvement) lymphocyte sensitization to BP is present though at a lower level than to EF¹. On theoretical grounds it would be expected that serotonin might block BP less well than EF. This was found to be the case (Table 1). Whilst serotonin reduced the result with EF from 16.3% to 1.1%, with BP it was reduced from 16.3% to 10.4%. It may be supposed that the antigenic determinant centred on tryptophan may also be present in BP but modified sequence on either side may result in a less close fit.

Circulating antibody studies have indicated¹⁵ that EF shared antigenic determinant(s) with purified protein derivative (PPD) of *M. tuberculosis*. Unpublished studies of lymphocytes from patients with severe active tuberculosis by the electrophoretic method have shown that these cells are sensitized to both PPD and to EF, confirming the original suggestion. The degree of sensitization to PPD (as shown by the percentage inhibition under the standard conditions of the cytopherometer test) always lay between 16 and 20% in all subjects examined. Serotonin was found to afford a partial block of PPD. In a typical experiment, for example, a subject giving 17.0% reduction with PPD, gave 17.9% if PPD was followed by serotonin (that is no change) but 14.0% if serotonin was added before PPD ($P < 0.001$). There would thus appear to be a descending order of "goodness of fit"; serotonin blocking EF most effectively, BP less well and PPD least, whilst other unrelated antigens are not blocked at all. Both BP and PPD are blocked by ALS and by concanavalin, again indicating that the mode of action of these two materials is different from that of serotonin.

These experiments support the concept that the encephalitogenic determinant is a binding site for serotonin and open up possibilities of using molecules of known shape as potential blocking agents to study the conformation of receptive antigenic determinants. A further series of drugs related to serotonin is under test.

We thank Dr F. C. Westall for a sample of synthetic (eleven amino-acid) peptide, and our clinical colleagues for access to their patients. The cytopherometers used were purchased by grants from the North East Multiple Sclerosis Society and the Multiple Sclerosis Research Fund Limited. P. R. C. holds a research fellowship of the Multiple Sclerosis Society of Great Britain and Northern Ireland.

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- Field, E. J., and Caspary, E. A., *Lancet*, ii, 1337 (1970).
- Caspary, E. A., and Field, E. J., *Ann. NY Acad. Sci.*, **122**, 182 (1965).
- Caspary, E. A., and Field, E. J., *Brit. Med. J.* (in the press).
- Carnegie, P. R., *Nature*, **229**, 25 (1971).
- Westall, F. C., Robinson, A. B., Caccam, J., Jackson, J., and Eylar, E. H., *Nature*, **229**, 22 (1971).
- Lennon, V. A., Wilks, A. V., and Carnegie, P. R., *J. Immunol.*, **105**, 1223 (1970).
- Field, E. J., Caspary, E. A., Hall, R., and Clark, F., *Lancet*, i, 1144 (1970).
- Field, E. J., and Caspary, E. A., *J. Clin. Pathol.*, **24**, 179 (1971).
- Caspary, E. A., *Nature*, **231**, 25 (1971).
- Caspary, E. A., and Field, E. J., *Brit. Med. J.*, **2**, 143 (1971).
- Caspary, E. A., Hughes, D., and Field, E. J., *Clin. Exp. Immunol.*, **7**, 395 (1970).
- Field, E. J., Ridley, A. R., and Caspary, E. A., *Brit. J. Exp. Pathol.*, **44**, 631 (1963).
- Caspary, E. A., and Field, E. J., *Europ. Neurol.*, **4**, 257 (1970).
- Bencina, B., Carnegie, P. R., McPherson, T. A., and Robson, G. S. M., *FEBS Lett.*, **4**, 9 (1969).
- Field, E. J., Caspary, E. A., and Ball, E. J., *Lancet*, ii, 11 (1963).

BOOK REVIEWS

Scientific Medicine

The Emergence of Scientific Medicine. By W. P. D. Wightman. (Contemporary Science Paperbacks No. 44.) Pp. 109. (Oliver and Boyd: Edinburgh, 1971.) 37½p.

ANYONE who has studied the reference literature of medical history will know how few books there are that really compel the reader to think beyond the commonplace. Dr Wightman's book belongs to this small group. Its text, based on lectures given over many years to medical undergraduates at Aberdeen, is dense and compressed with not a wasted word. In these days of verbosity it is to be welcomed for that reason alone.

More than anything, Dr Wightman dislikes the kind of prejudices that spring from perverted logic or ignorance of history. One of his particular hates is the fallacy that the Church impeded the progress of science and medicine in the Middle Ages and he quite obviously takes the greatest pleasure in setting the record straight, following the lead set by Dr Talbot in his *Medicine in Mediaeval England* (1967). He also delights in shaking us out of our arrogance, as for instance when he claims that a belief in the procedures of temple incubation is not *ipso facto* irrational "provided that . . . the opinion . . . as to right action . . . is based on a conscious relation of ideas".

His chief concern is with plotting the course of non-magical attitudes to the body, from Greek times to the seventeenth century. This he does by interposing his own comments between cleverly chosen extracts from the works of a dozen or so seminal writers. Here he explains their apparent contradictions; there he marks out the line that divides true originality from high competence. No hero worshipper, he gives credit to Aristotle for "imposing system upon the flux of experience" and yet recognizes, in respect of logic, the philosopher's debt to his teachers and predecessors; while Galen is praised not only for his experimental genius but in addition for the persuasive consistency of his humoral theory. In a sense, as Wightman points out, the latter can be said to prefigure the modern theory of endocrine balance.

As for Harvey, even he, in his Aristote-

telean view of the blood, is shown to be a product and disciple of tradition; whereas that sworn enemy of the orthodox, the "God-intoxicated ranter" Paracelsus, emerges almost as a blood brother of Francis Bacon, at least insofar as both men followed similar methodical lines of thought. Again, Fracastoro was a scientist in the modern mould, not so much because he formulated a contagion theory as because of the "critical manner in which he confronted theory with facts".

This short book should be read twice over, and not only by newcomers to the subject. It helps us to glimpse some of the conceptual difficulties that faced our forebears and it arouses admiration for the experimental methods evolved by men such as Richard Lower.

E. GASKELL

On Being Neurochemical

Methods in Neurochemistry. Edited by Rainer Fried. Pp. xi+374. (Marcel Dekker: New York, 1971.) n.p.

THIS book's preface claims neurochemistry as one of the fastest growing of the life sciences, and this is probably true: the *Journal of Neurochemistry* is now as large as was the *Biochemical Journal* of a previous generation. The contents of *Methods of Neurochemistry* suggest that one of the methods of growth of the subject is by capture. It is to cover "all important areas of the neurosciences, mainly in the fields of biochemistry and pharmacology". Of the book's six chapters, that by Donough O'Brien on biochemical screening in mental retardation could equally count as clinical biochemistry; while that of Krnjević on microiontophoresis embraces much physics and pharmacology. The neurochemical aspect of each contribution is nevertheless undoubted; the neurosciences by their nature encourage interdisciplinary work and thinking. Thus O'Brien's account, which is the largest in the book, gives both clinical and technical commentaries on some twenty metabolic abnormalities in man which are related to mental dysfunction. It lists briefly seventy to eighty such conditions and avoids discussion of those which are adequately recounted in books of reference; it also demonstrates well the need for expanded laboratory facilities in this field.

The stated editorial intentions regarding *Methods in Neurochemistry* are to present several distinct areas of research, rather than obey a unifying concept, and the book begins with accounts of the purification and properties of myelin isolated from neural systems (L. C. Mokrash), and of the purification and properties of phospholipids (G. B. Ansell and S. Spanner). Methods of determining catecholamines and their metabolites are also described (D. F. Sharman); the three contributions include excellent experimental details.

Of very different character is the final and second largest contribution, which is by the editor and a co-worker and is a tabulation of "compounds of importance to neurochemistry". Errors in this section include the relatively harmless mis-spelling of names of substances, but also mistakes among less well known compounds. Saccharopine, an amino-acid derivative found as a urinary constituent in a form of mental disorder, is given the structural formula of a C₁₀ compound and the molecular formula of a C₁₁ compound. Three successive entries are: chlorpromazine, depicted with triply-bonded chlorine in a 6-membered ring; cholesterol with structure lacking angular-bonded methyl groups; and cocaine, with the molecular formula of a C₁₇ compound and the structural formula of a C₁₈ compound. Neurochemistry: important, expanding, should be able to command better service.

H. McILWAIN

Blastocyst Biology

The Biology of the Blastocyst. Edited by R. J. Blandau. Pp. xi+560. (University of Chicago: Chicago and London, March 1971.) £12.40.

THE current publication explosion has produced torrents of books on almost every conceivable aspect of reproduction. Prospective readers are thus likely to scan new books very carefully for quality, price and comprehension before being tempted to invest their money. *The Biology of the Blastocyst* sets a high scientific standard, covers a comprehensive set of topics (perhaps too wide) and has a price that even by current standards is not cheap. It is obvious that this book will prove of considerable value to the rapidly in-

creasing number of researchers engaged in reproductive physiology. It should serve as a valuable reference for data for some years before it is inevitably superseded by a later publication.

Reports of conferences seldom debate in detail a major advance in a particular topic; they tend to cover broad aspects of a subject, with chapters penned by authorities in these different areas. This book is no exception, for there are more than thirty authors involved in thirty-one chapters. In this area of biology the comprehensive description of the subject matter demands an interest in a wide variety of subjects such as endocrinology, differentiation, ultrastructure, tissue culture, immunological reactions between mother and foetus, teratology and uterine musculature. At implantation, as at certain other stages in pregnancy, many events occur simultaneously in the embryo and the mother. There can be no doubt that a full and fair treatment of these events is provided in these chapters, which are well written by authorities in each topic. Naming names would be tedious and unfair: let it suffice to state that almost every conceivable topic touching on the blastocyst is covered somewhere in the volume. Inevitably some authors have chosen to write a brief review of their topic while others have described their own experiments, and this inevitably leads to some unevenness in the book. Yet every chapter offers a wealth of detail or something interesting and stimulating, and it is a pleasure to browse through the pages, picking up ideas or comments. Discussions in the conference have been omitted, which is in many respects a relief from pet ideas or theories unsupported by critical material or analysis.

My major criticism concerns the repetition of subject matter in various chapters. This repetition obviously permitted more contributors to state their views in the symposium, but the combination of some chapters or the exclusion of others could have provided a more streamlined presentation. Some of the chapters contain material of peripheral interest, such as fertilization or the biochemistry of the cleaving embryo, and one chapter could have sufficed for these. Other chapters are somewhat superfluous because two or more contributors cover essentially the same subject matter, for example, there are three on immunology. More rigorous selection of speakers, or even joint chapters, would have led to a more concise and easily presented book.

The book is well produced in a handsome size, contains detailed references and is well illustrated. The editing has been done with care. It will prove a most valuable source of material.

R. G. EDWARDS

Bioelectromagnetism

Electromagnetic Fields and Life. By A. S. Presman. (Translated from the Russian by F. L. Sinclair.) Edited by Frank A. Brown, jun. Pp. xix+336. (Plenum: New York and London, 1970.) \$25.

EVER since Franz Mesmer, the Austrian doctor and mystic, started some two hundred year ago to treat patients with iron magnets, controversy has continued over the question of whether magnetic fields can affect biological systems. In this book A. S. Presman, a biophysicist at Moscow University, sets out to review the experimental evidence for biological effects of electromagnetic fields with frequencies below 10^{12} Hz (thus excluding visible and shorter wavelengths, where quantum effects are important) and proposes a new hypothesis to explain them.

In the first part of the book the author describes the naturally occurring electromagnetic fields to which all organisms are continuously subjected. The electrical properties of living organisms are discussed briefly, and various physical mechanisms by which such fields could affect biological systems are listed.

In the second part the author reviews the experimental evidence for electromagnetic field interactions. Especially useful is the extensive bibliography, although references are only listed up to 1967. The principal effect is heating of tissue which leads to death if the heat cannot be quickly dissipated. Surprisingly, however, effects seem to occur at field intensities which are too low to cause appreciable heating. One reads, for example, of the good correlation between the incidence of cerebrospinal meningitis in New York and solar activity, and between magnetic storms and mortality from nervous and cardiovascular diseases in Copenhagen and Frankfurt. Planaria, bees, wingless termites and many flies can orientate themselves relative to the Earth's magnetic field. Seeds of corn and wheat sown with the radicle pointing south germinate one to three days earlier than those with the radicle pointing northwards, though the effect apparently depends on the phase of the Moon. Magnetic fields of greater intensity (120 kg) prove lethal to *Drosophila* if exposed for sixty minutes, while a combination of magnetic field (600 g) and microwaves is very effective in treating tumours in mice.

The fact is, of course, that no known physical theory can explain these low-intensity phenomena because the energy involved in the fields is far too small. There are thus two alternatives: either the experimental results are inconclusive, and the statistical correlations are

misleading, or, alternatively, new processes are occurring about which nothing is yet known. Presman argues that the latter is the case, and thinks the evidence indicates a transfer of information from the environment to the organism which is independent of the amount of energy carried by the waves. The field may produce its effect by modifying the subtle neural interactions between the various parts of the organism, although there are no clues yet as to how this might be accomplished.

Finally, Presman speculates on the consequences of his hypothesis. Animals, for example, may be able to communicate using electromagnetic signals, and the simultaneous changes in the direction of motion of birds in flocks and fish in shoals could be initiated in this way. Eels may sink to great depths in the Sargasso Sea to protect their young from terrestrial electromagnetic waves, and it may prove necessary to protect hospital patients from such fields using suitable metal screens. Intriguingly, telepathy and other parapsychological phenomena may at last have a physical foundation.

It would be pointless to quibble over the many controversial statements made in any book as speculative and wide ranging as this. Two points should be mentioned, however. There is an error in the equivalent circuit of a typical cell (page 35) which leads the author to conclude, wrongly, that no electric current flows through a cell in a static electrical field. Second, the statement (page 85) that the minimum theoretical electric field strength which can affect a biological system is 10^6 V m⁻¹, is difficult to reconcile with the fact that such a field, established in normal pond water, would cause it to boil within 0.01 seconds, and would rapidly be detected by any biological system. Nevertheless, this is an interesting and readable book, and "the author's modest hope, that the ideas which he presents will stimulate further critical discussion and researches" should certainly be fulfilled.

ALUN M. ROBERTS

Living Together

Aspects of the Biology of Symbiosis. (Proceedings of a Symposium held in Boston.) Edited by Thomas C. Cheng. Pp. x+327. (University Park: Baltimore; Butterworth: London, July 1971.) £7.

THE title of this book might mislead some people into believing that this is a text-book, but it is not. It is a collection of twelve papers given at a symposium of the American Association for the Advancement of Science in December 1969.

There is no universally agreed definition of the term "symbiosis". Some

restrict its use to clearly mutualistic associations, while others extend it to include many examples of parasitism and commensalism. This book tends to the latter use of the term, and its bias is strongly zoological, as exemplified by the selection of topics discussed: penetration stimuli for schistosome cercariae; schistosomules and mouse skin; communication between fishes in cleaning symbioses; nubibranch associations as symbioses; respiratory metabolism of a trematode metacercaria and its host; enhanced growth and shell deposition in parasitized molluscs; a new hypothesis on the goodness of parasitism; nutritional aspects of a symbiosis between a crab and a Hawaiian sea urchin; effect of external factors on host-symbiont interactions in green Hydra; blue-green algae in echiuroid worms; immunity in invertebrates and protection of anemone fishes from sea anemones.

The best aspect of this book is that all the articles are concerned with the experimental study of symbiosis, and mostly lack the rambling, vague speculation that has bedeviled some of the earlier writing about the subject. As in most books of this type, the articles are rather uneven in quality. For example, it is most surprising that the article on nudibranchs does not mention chloroplast symbiosis. The very long article by Lincicome on the experimental evidence for his hypothesis on the goodness or parasitism certainly presents a concept new and unfamiliar to most biologists concerned with symbiosis: "Parasitism, one of life's great phenomena, possesses a quality of goodness that has largely been overlooked because man has been so possessed with the disease aspects of this association". Kawaguti's very short but fascinating paper on blue-green algae in echiuroid worms states, in the discussion and conclusions, that *Ikedosoma* can consume ammonium and that its proboscis has a reduced oxygen consumption. Unfortunately, the experimental basis for such a statement is not mentioned in either the results or the materials and methods sections, both being exclusively concerned with microscopy. There are some articles, however, such as that by Lytle, Epp and Barthalmus on green Hydra, which manage to maintain a uniformly good standard of competence.

Most of the articles in this book are concerned chiefly with describing the results of research into rather narrow fields. I am therefore perplexed that the authors publish them in this book rather than in a journal. Books take much longer to be published than journals, and this can be important in fields which are rapidly advancing. Further, some of the articles (and

hence, the reputation of their authors) would have been noticeably improved by the rigorous refereeing characteristic of the better journals, but lacking in most books of this type.

In spite of the flood of recent research into symbiosis, the really good book on the subject remains to be written: perhaps the subject has now grown so large that such a book has become impossible to write.

D. C. SMITH

Electrochemistry Now

Modern Electrochemistry: an Introduction to an Interdisciplinary Area, Vol. 1. By John O. M. Bockris and A. K. N. Reddy. Pp. xxxii+622. (Plenum: New York, 1970.) \$19.50.

Modern Electrochemistry: an Introduction to an Interdisciplinary Area, Vol. 2. By John O. M. Bockris and A. K. N. Reddy. Pp. xxvii+623-1432. (Plenum: New York, 1970.) \$19.50.

THE publication of another book by Professor Bockris is an event in the world of electrochemists and the present large two-volume effort, with Dr A. K. N. Reddy as co-author, certainly justifies the interest which it is commanding from that fraternity. The latter may have expected something rather different and it is important therefore to establish at the outset the purpose and philosophy of the book. Certainly it is not a research treatise, it is not a chemists' handbook to the theory and practice of electrochemistry, it is not even perhaps a chemistry students' text. What is it? The answer is contained in the sub-title and preface—"An Introduction to an Interdisciplinary Area" and "... a book which outlines the field at a level which can be understood by people entering it from different disciplines ...". The authors see electrochemistry not as a branch of chemistry but as an emerging discipline groomed to the present age in which the solution to the problems of pollution and power supply together with so many appurtenances of modern civilization will be electrochemical. To this end "modern electrochemistry" emerges as a fusion of ideas at present scattered throughout the various branches of physical science, a fusion albeit at a comprehensive theoretical level. Thus the first volume deals essentially with theoretical considerations leading up to and relating to physical processes within the electrolyte phase. It includes structural and non-structural treatment of ion-solvent interactions, solvation numbers, dielectric constant of water and ionic solutions, ion-ion interactions including the Debye-Hückel equations and extensions, activity coefficients, ion-transport (including the laws of diffusion, and

Laplace transforms), conduction and ion migration, and concludes with chapters on protons in solution and ionic liquids (fused salts, liquid oxides, and so on). The second volume has electrode phenomena ("electrodics") as its principal theme and covers the establishment, thermodynamics and structure of electrified interfaces, charge-transfer processes of simple and complex electrode reactions and electrode kinetics. The final two chapters deal with electrode reactions of special interest (for example, electro-catalysis, electro-crystallization) and those of technological importance (such as corrosion and electric power storage).

The electrochemist will undoubtedly appreciate having together in one publication so much of the fundamentals of his subject, but more so perhaps from the point of view of refurbishing his basic knowledge than in providing any detailed information on current research methods and practices. Thus there is no reference to a.c. impedance techniques and only brief reference to galvanostatic, potentiostatic and other experimental methods. Polarography is represented chiefly by a derivation of the Ilkovic equation, and present day amperometric and chronopotentiometric methods also receive scant attention. The absence of reference to practical procedures is a notable omission in a work of this importance, even accepting the authors' concentration on fundamentals, and we feel that a better balance would have resulted from such inclusion at the expense of many fringe topics. The inclusion of a section on batteries and fuel cells fits in with the authors' aims, but is too brief to do more than scratch the surface of one of those areas which is the *raison d'être* for the books. Even more surprising is the relegation of the section on galvanic cells to quite late in the second volume, where it receives rather limited selective treatment, a result no doubt of the constraint imposed by the authors' electrolyte-electrode division of the subject.

The bulkiness of the two volumes is in large measure a result of the praiseworthy efforts of the authors to provide the maximum of help to the reader, and this is exemplified by numerous informative diagrams, frequent essay-type summaries and a question/answer presentation. To the more advanced reader, however, much of this might well be regarded as seeking clarity at the expense of brevity and such readers may now, like us, hope one day to see a more succinct condensed version. This style of presentation is of course shared by other American writers of textbooks and is therefore more likely to be acceptable to readers from the United States. Certainly it increases the price to a level which must make possession

by most undergraduates unlikely and possibly by many postgraduate workers too. Words like "electronation" (for cathodic reduction) irritate but neither this, nor our other criticisms, should mask the fact that the authors have produced the most comprehensive theoretical text so far, quite apart from their thesis that electrochemistry should be regarded as a new interdisciplinary subject. The two volumes are a must for any university/research library and those individuals who might be able to have their own copies should be agreeably satisfied with their purchase.

R. F. PHILLIPS
N. A. HAMPSON

Learning in Bed

Learning and Sleep. By F. Rubin. Pp. xi+138. (John Wright and Sons: Bristol, June 1971.) £1.75.

If one mentions "sleep learning" to an academic psychologist one is likely to meet the same sort of defensive disbelief elicited by ESP. Fifteen years ago Simon and Emmons established that retention of material presented to a sleeping person is negligible unless he is in only very light sleep. Since then, outside the Soviet Union and Eastern Europe, the idea of sleep learning has been treated with scepticism, and criticism of studies has almost entirely been centred on electro-encephalographic evaluation of whether subjects were "really asleep". The baby has been thrown away with the bathwater: the idea that people might be able to learn in bed, at times when they are not soundly asleep, has been almost entirely ignored by professional psychologists.

This book therefore may serve an excellent purpose, bringing to the attention of those involved in teaching language courses, for example, the possibility of facilitating learning while we are in bed and not yet sound asleep. The opening chapters dispel misconceptions many people have about hypnopædia (the term the author prefers). No wild claims of using all eight hours of sleep are made: learning is restricted to a period immediately after retiring and shortly before rising in the morning. The author lays great stress on the fact that here is no quick, easy short cut to knowledge. He discusses at length Soviet and Czech experiments (chiefly in language tuition), which present words or phrases, with their mother-tongue equivalents, to highly motivated students who simultaneously receive daytime tuition in the language. The crucial point is that the presentation of material during the drowsy state can facilitate learning, in properly structured courses. The experiments he describes have often not been rigor-

ously designed: only three experiments with control groups are reported; nevertheless, the theory and evidence are sufficient to warrant practical tests of the scope and usefulness of hypnopædia in the West.

A paperback aimed at the popular market would seem a good way of putting these points across: a pity therefore the book is priced so high. There are other annoying things, such as a pair of tables describing sleep stages which are blatantly inconsistent (pages 19 and 21). This might not worry the casual reader, but there are more infuriating points: at times the author hardly makes sense (page 39, for example), and he is fond of quoting and re quoting references, as if to impress the reader. The organization of the book is particularly frustrating. The fourth chapter is a study by the author which belongs with the other experiments discussed at the end of the book. Why does he proceed to practical details of which tape recorders to use and how to intone lessons in the fifth chapter only to return to the experimental evidence that sleep learning really works in the eighth chapter?

In spite of these serious faults I would still recommend this book to those not prone to irritability, who want a fresh look at a subject neglected by "serious" psychologists.

HUGH FIRTH

Underwater Acoustics

Underwater Acoustics. Edited by R. W. M. Stephens. Pp. vii+269. (Wiley Interscience: London and New York, December 1970.) £6.75.

THIS book comprises eight articles on various aspects of underwater acoustics, which are based on a series of lectures given at Imperial College, London, in 1967. The contributors are either physicists or engineers who are actively engaged in research in the different specialist topics presented in the book.

The purpose of the introductory chapter by the editor is to provide the reader with background information on the sea as an acoustic medium. In thus setting the scene for the specialist contributions that follow on topics such as sound velocity, sound attenuation, noise, reverberation and cavitation are briefly reviewed.

The first specialist chapter is a mathematical study of the propagation of acoustic waves underwater, when the effects of the medium boundaries and the medium's velocity profile are included. The limitations of solutions by ray theory methods in these circumstances are discussed. The approach favoured by the author is the method of normal modes which arise from the solution of the wave equation in cylindrical co-ordinates with the appropriate

boundary conditions applied. The general complex solutions obtained are presented and discussed in some detail. Space is also devoted to the "not so spherical cow", that is the effects on the solutions when factors such as the "real" sea surface and bottom cause departure from the mathematical model analysed. The sea surface is in general nonspecular in its behaviour, and so the next chapter, which is an authoritative review of the back and specular scattering of acoustic waves from the sea surface, follows quite naturally. The discussion involves a fair degree of mathematical content, the scattering theory being developed from the Helmholtz-Kirchhoff integral. Frequent comparisons are made between theory and experimental results. A comprehensive reference list is also provided.

The fourth chapter is basically a review of the sonar equation and some pertinent aspects of signal processing in relation to acoustic exploration. In particular the discussion is based on the recognition differential and noise terms in the equation. Factors such as detection threshold, processing gain and visual integration are briefly examined. A fairly detailed review on background noise is presented which includes such aspects as reverberation, self noise, cavitation noise and ambient noise. The next chapter may be regarded as based on the target strength term in the sonar equation. It is essentially a detailed and lengthy review of experimental results associated with the echoes from simple geometrical objects and from more complex objects, in particular—fish. The information presented is extensive and the topics covered too numerous to detail here.

Non-linear acoustics form the subject of the sixth chapter. The emphasis is on the basic physical mechanism behind the non-linear effects within the range of validity of perturbation theory. A brief discussion of some of the applications of non-linear effects in underwater acoustics is also included. The next chapter is a review of underwater instrumentation. Subjects briefly discussed include transduction, telemetry, wave measurement and acoustic navigation systems. The final chapter is concerned with the audio communication between free divers. Physiological factors, such as hearing and word formation are discussed in addition to a number of the technical problems.

Although an attempt is made to retain a theme throughout the book, probably because of the diverse authorship, the book in general is a mixture. Consequently it is difficult to see for whom the book is intended. A number of the chapters are suitable for the more general reader but the remainder are specifically aimed at the research worker.

B. V. SMITH

CORRESPONDENCE

Elitism in Astronomy

SIR,—I have followed the various discussions about British astronomy, and have been surprised that the chief factor we would consider in the US has remained unmentioned in public.

I refer of course to a critical number of first rate scientists. Between the two Royal Observatories and Pretoria, as an outside observer I count six first rank astronomers. In Pasadena associated with the Hale Observatories, I count fourteen astronomers of distinctly higher average performance. The UK group is split between three locations and three establishments. In quantity of first rank people at one location, all three are operating at a sub-critical level.

It has been suggested that to set up a national centre with a critical number of high calibre astronomers is elitism. The question is not whether the UK wants an elite. It is whether it wants one place alive, or many half dead. Such a centre would not be a third centre. It would be a first centre. Its physical location should in the absence of special constraints be preferably chosen on logistical grounds. But the good reasons for avoiding existing institutions are that the petty inter-institutional squabbles and prior staff commitments of existing institutions might create a sickening home for a healthy baby organization.

Yours faithfully,

NEVILLE J. WOOLF

*Director,
University of Minnesota Observatories,
Minneapolis, Minnesota 55455*

Conquest of Cancer

SIR,—I would like to clarify some aspects of the Conquest of Cancer Bill, S.1828, recently passed by the Senate and now awaiting House action. As the director of a cancer hospital and research institute, I am vitally interested in the conquest of cancer, and as a member of the National Panel of Consultants appointed by the US Senate, I participated in the recommendations that led to S.1828.

Unfortunately, there is considerable confusion about the provisions of S.1828. This confusion, understandably, comes about because S.1828 evolved from several other bills and proposals with identical or similar names.

S.1828 provides for increased emphasis on cancer research within the framework of the National Institutes of Health. The National Cancer Institute will remain a part of NIH, but will have

greater autonomy. It will not become a separate agency. Nor will its relationship to NIH be a "name only" affair as has been mistakenly stated by those who confuse it with other proposals. Under S.1828, the National Cancer Institute remains in its own buildings on the NIH campus. The NCI personnel will continue to interact with the other institutes within the NIH. The NCI will continue to use the Clinical Center of NIH for studies on patients. It will continue to use the NIH computer facilities and the NIH laboratory animal breeding and holding facilities just as it does today. The NCI will continue to use the NIH study sections for peer review of research grant applications, but will be able to supplement them with additional study sections concerned with specific aspects of cancer. For example, I hope to see a breast cancer study section added. The major change in the relationship of the Cancer Institute involves greater budgetary and planning freedom. Instead of the present arrangement that requires six (6) layers of officials above the Cancer Institute to study and approve all major plans and budgets, the Director of the Cancer Institute will report directly to the President. This administrative change will speed up the conquest of cancer considerably.

In every other respect the Cancer Institute will retain its present connexions with the other institutes and with NIH.

Furthermore, to be certain of good coordination with the NIH and other government agencies, S.1828 provides that the Advisory Council of the Cancer Institute must include, as ex-officio members, the director of NIH, the Secretary of Health, Education and Welfare, and the director of the Office of Science and Technology.

Some charge that S.1828 will break up the National Institutes of Health. But NIH is not so fragile that a change to increase efficiency and give greater autonomy to an operating institute will destroy it. Businesses and other government agencies are frequently reorganized for greater efficiency.

Another criticism is that other Institutes within NIH will want similar arrangements for themselves. Perhaps they will. Let's try it with one: the Cancer Institute. If cancer research moves ahead more rapidly under the new arrangement (as most of us who studied the situation believe it will), and if other institutes then wish to have a similar arrangement, why not give it to them? After all, we are primarily concerned with improving the people's health, not with maintaining

any specific administrative pattern. If, on the other hand, cancer research does not progress more rapidly under the new arrangement, the other institutes would not want to copy it.

Some fear that the increased emphasis on cancer research implies a promise of a cure for cancer in a few years. No one associated with S.1828, the National Panel of Consultants, or the American Cancer Society has ever made or implied such a promise. We do believe that the time has come for an accelerated and intensified assault on cancer, and that the eventual conquest of this disease can be brought nearer.

Would passage of S.1828 result in neglect of basic research? Exactly the opposite is true! S.1828, and the Report of the Panel of Consultants both call for expanded basic research.

Finally, there is the point that the bill cannot guarantee a cure for cancer. There is no doubt about that and, indeed, no one would suggest there is a guarantee of a cure for cancer in any kind of legislation. There is the assurance in this bill that progress will be faster and that the cures will come sooner than they would without it. That is reason enough for its enactment and that is the reason my colleagues and I support it.

Yours faithfully,

SOLOMON GARB

*American Medical Center at Denver,
Spivak, Colorado 80214*

Research and Development

SIR,—Your review of Dr Duncan Reekie's survey for the Centre for the Study of Industrial Innovation of abandoned industrial R and D projects complains that it is "rich in detail and poor on analysis and generalization". We would be the first to admit that the survey is short on the kind of loose ill-defined and often unsupportable generalization from which the discussion of innovation greatly suffers. Your reviewer provides a number of new illustrations—for example, that "even successful research and development projects in Britain are understaffed". One major object of the centre is to study in detail what happens in firms so that general conclusions can rest on an adequate basis of knowledge. It is perhaps because firms realized the relevance of this approach (rather than because of "luck" as your review suggests) that they cooperated so fully in Dr Reekie's study.

Your reviewer writes as though the report suggests that projects should not be abandoned. On the contrary, it refers to the inevitability of this. But your review is altogether too complacent to claim that none of the cases Dr Reekie reported should cause surprise and that all of them merely represent "unavoidable ups and downs of commercial life". In no fewer than seven of the twenty firms visited development projects had been abandoned because they had been unwittingly duplicated within the same organization. In other cases, there had been several years of successful technical development before any market assessment had been undertaken. Surely no management could claim that such cases were unavoidable? In regard to the timing of abandonment your reviewer appears to agree with the centre—though with an odd twist he suggests that he is saying something different.

We appreciate your having paid so much attention to our first report. But we must advise those of your readers who wish to know what the report says to read it in the original.

Yours faithfully,

DUNCAN BURN

Chairman,
Centre for the Study
of Industrial Innovation,
162 Regent Street,
London W1R 6DD

Those who collect information but who neglect to analyse it must surely not be surprised if others draw lessons from it which may sometimes be unwelcome—Editor, *Nature*.

Erratum

Last week's News and Views editorial "Haemoglobin Synthesized in *Xenopus* Eggs" (233, 161; 1971), contained the

erroneous statement that AUGpolyU is translated when injected into *Xenopus* eggs. Of course, as Gurdon *et al.* reported in their article on page 177 of that issue, this messenger does not promote the incorporation of phenylalanine.

British Diary

Tuesday, September 28

Centralized Control Systems (four-day conference) Institution of Electrical Engineers; and the Institution of Electronic and Radio Engineers, at Savoy Place, London WC2.

Schizophrenia: Biochemistry, Genetics and Therapy (two-day conference) Schizophrenia Association, at Church House, Dean's Yard, Westminster, London SW1.

Wednesday, September 29

Less Common Methods of Separation (9.30 a.m. symposium) Institution of Chemical Engineers, Midlands Branch, at the University of Birmingham.

Microbial Control in Pharmaceutical and Cosmetic Preparations (two-day symposium) Society of Cosmetic Chemists of Great Britain, jointly with the Pharmaceutical Society of Great Britain, at Imperial College, London SW7.

Thursday, September 30

Biochemical Engineering (2 p.m. symposium) Institution of Chemical Engineers, at the University of Technology, Claverton Down, Bath.

Friday, October 1

Autumn Meeting and Annual General Meeting (afternoon) Bone and Tooth Society, at the Royal Dental Hospital, London.

The Properties of Anticorrosive Pigments (10 a.m.) Mr J. Bowden, Oil and Colour Chemists' Association, Scottish-Student Group, at the St Enoch Hotel, Glasgow.

Saturday, October 2

Teaching of Mathematics in Schools in Relation to the Teaching of Physics (symposium) Institute of Mathematics and its Applications, jointly with the Institute of Physics, at Eton College, Windsor.

Monday, October 4

Practical Pigment Dispersion (7 p.m.) Mr J. R. Groom and Mr M. Baker, Oil and Colour Chemists' Association, at the Queens Hotel, George Street, Hull.

Reports and Publications

not included in the Monthly Books Supplement

Great Britain and Ireland

An Index to the Systematic Collection of Minerals in the British Museum (Natural History). Twentieth edition. Pp. 39. (London: British Museum (Natural History), 1971.) 20p. [96]

University of Bristol, Department of Agriculture and Horticulture. Long Ashton Research Station—The National Fruit and Cider Institute. Report for 1970. Pp. xv+208. (Long Ashton, Bristol: Long Ashton Research Station, University of Bristol, 1971.) £1.25. [96]

Plant Breeding Institute, Cambridge. Annual Report 1970. Pp. x+126. (Cambridge: Plant Breeding Institute, 1971.) 75p. [106]

Agricultural Research Council. Meat Research Institute—Annual Report 1969-70. Pp. 80. (Langford, Bristol: Meat Research Institute, 1971. Obtainable from HMSO.) 89p net. [106]

Myth, Magic and Archaeology. By Professor F. J. Tritsch. (Inaugural Lectures delivered on 8 December 1970.) Pp. 20. (Birmingham: The University, 1971.) 15p. [106]

Building Research Station Digest, No. 130: Drainage Pipelines—1. Pp. 8. (London: HMSO, 1971.) 5p. [116]

University of Strathclyde. Annual Report 1969/1970: Part 1: Pp. 1-130. Part 2: Pp. 131-193. (Glasgow: University of Strathclyde, 1971.) [116]

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